THE TEXT IS LIGHT IN THE BOOK

NATIONAL
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OF THE UNITED STATES
OF AMERICA

BIOGRAPHICAL MEMOIRS

VOL. XIX

CITY OF WASHINGTON
PUBLISHED BY THE NATIONAL ACADEMY OF SCIENCES
1938

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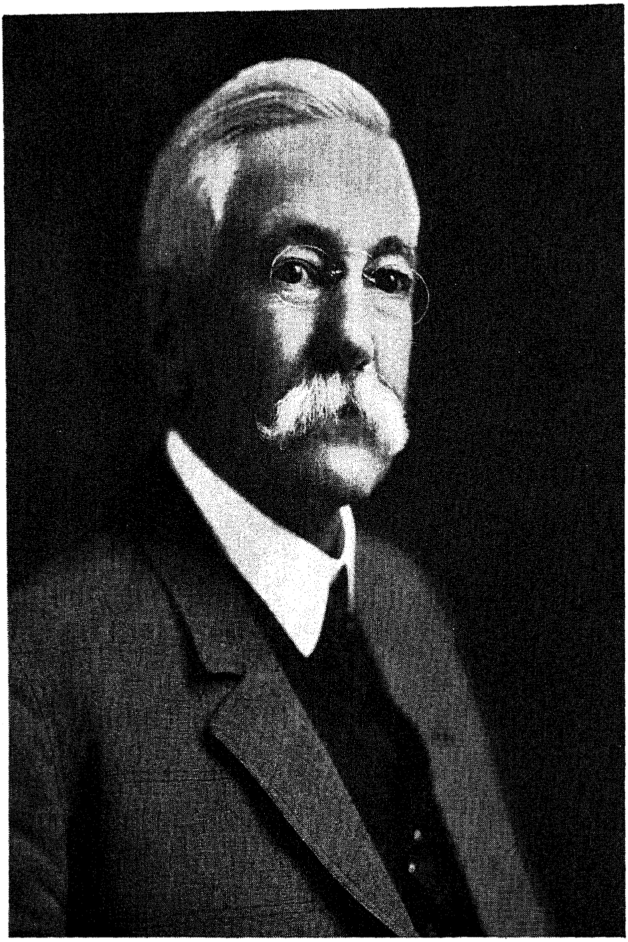
CONTENTS



Robert S. Woodward

NATIONAL ACADEMY OF SCIENCES
OF THE UNITED STATES OF AMERICA
BIOGRAPHICAL MEMOIRS
VOLUME XIX—FIRST MEMOIR

BIOGRAPHICAL MEMOIR

OF

ROBERT SIMPSON WOODWARD

1849–1924

BY

F. E. WRIGHT

PRESENTED TO THE ACADEMY AT THE AUTUMN MEETING, 1937

ROBERT SIMPSON WOODWARD[1]

1849–1924

BY F. E. WRIGHT

A thorough knowledge of mathematics and physics is an excellent introduction to creative work in astronomy, geology, and engineering. In the early days of the National Academy of Sciences this close connection between mathematics and physics, on the one hand, and astronomy, geology, and engineering, on the other, was recognized in the names of the Sections to which Academy members were assigned. Thus Dr. Woodward belonged for many years to the Section of Mathematics and Astronomy and to the Section of Physics and Engineering. He was one of a group of investigators whose interest was primarily in mathematical physics; but who, in his own words, realized that "the earth furnishes us with a most attractive store of real problems"; that "its shape, its size, its mass, its precession and rotation, its internal heat, its earthquakes and volcanoes, and its origin and destiny are to be classed with the leading questions for astronomical and mathematical research." To these problems men like Laplace, Fourier, Gauss, G. H. Darwin, and Lord Kelvin devoted much attention, and in so doing advanced both geology and astronomy and their own mathematical physical sciences.

Dr. Woodward's contributions to geology were in the domain of geophysics, and, although his articles were printed many years ago, they still rank as the most important papers on these subjects thus far published in America. His influence, moreover, in stimulating and encouraging geological work and, in his later years, as president of the Carnegie Institution of Washington, in organizing and administering research projects in geophysics and astrophysics has been an important factor in the progress of geology, astronomy, and other sciences.

Robert Simpson Woodward was born on July 21, 1849, at Rochester, Michigan; he died on June 29, 1924, at Washington,

[1] Revision of "Memorial of Robert Simpson Woodward," by Fred. E. Wright, published in the Bulletin of the Geological Society of America, Vol. 37, pp. 115-134, 1926.

D. C., at the age of nearly seventy-five years, after a long period of illness and suffering, following an attack of influenza.

His early life was spent on a farm near Rochester, a village in Oakland County about 30 miles north of Detroit. His father, the Hon. Lysander Woodward, was one of the most progressive farmers in the State; he sought to apply scientific principles to the operation of his farm and took a keen interest in public affairs. Farm land in this part of the State is of excellent quality and responds well to proper treatment. Without question, this attitude of mind of the father was passed on to the son, who ever afterward was an enthusiastic student of farming methods and in later years acquired and successfully operated, for recreation, a farm in Montgomery County, Maryland, not far from Washington. The following biographical sketch of his father, given in C. R. Tuttle's General History of the State of Michigan, Detroit, 1873, pages 167-168, is significant:

"Lysander Woodward, one of the most prominent men in Oakland County, was born in the town of Columbia, Tolland County, Connecticut, November 19, 1817. His parents, Asahel Woodward and Harriet House, were natives of that State. In 1825, with his parents, he removed to the town of Chili, Monroe County, New York. From here he emigrated to Michigan in the fall of 1838. He married Miss Peninah A. Simpson on the 11th of May, 1843, and settled near the village of Rochester, Oakland County, Michigan, where he still resides. Mr. Woodward's chief occupation is that of a farmer, but he has held many important offices in his township. In 1860 he was elected representative from the first district of Oakland County to the State Legislature, and served with considerable distinction during one regular and two extra sessions. He was county treasurer of Oakland County two terms and performed his duties in a thoroughly satisfactory manner. Mr. Woodward was also president of the Oakland County Agricultural Society for three years, and in this position did great service in advancing the agricultural interests of the Detroit and Bay City Railroad and has been instrumental in canvassing for and promoting its construction. He was chosen the first president of this company in 1871, which important office he held up to May 15, 1873, and he still remains one of the directors of the company. Mr. Woodward owns one of the largest and best cultivated farms in Oakland County."

Doctor Woodward's mother belonged to the Simpson family, a name prominent in the annals of Connecticut. From his mother he inherited many genial traits of character and an interest in

his fellow-man, traits that were an integral part of his personality.

His boyhood days were passed in an atmosphere of pioneer farm development, coupled with a broad interest in public affairs. His school training was excellent. The Rochester Academy which he attended ranked well among the schools of the State, and his teachers, especially one, were better than the average. On finishing the academy he wished to go to the university, but his father was not in sympathy with university training and only after the most earnest appeals was he persuaded to allow his son to attend. Four years later, in 1872, the son graduated from the University of Michigan with the C. E. degree. Several summers during the college period were spent in field-work as aide on the United States Lake Survey. After graduation he became assistant engineer on the United States Lake Survey and retained this position for ten years, until 1882, when the work was practically finished.

During this period he was engaged chiefly in primary triangulation work on the Great Lakes, under the leadership of General Comstock of the Corps of Engineers, War Department. This gave him a most thorough training in primary triangulation, in latitude and longitude determinative work, in the testing of field instruments and field methods, and in the office adjustment of field observations by least-square methods. The results of this work were published in chapters 16 to 20 and 24 and 25 of General Comstock's Monographic Report on the Survey of the Lakes, Professional Paper No. 24 of the U. S. Army Engineers; also in several articles in technical journals. During this period Dr. Woodward acquired an interest in the earth as a whole—in its shape, its tides, its atmosphere, and in the host of geophysical problems, many of which still await solution. This keen interest was maintained throughout his life and led him during the next decade to investigate some of the outstanding geophysical problems, and to solve them in spite of formidable mathematical difficulties. To him they had an irresistible fascination, and in later years he looked eagerly forward to the time when he could lay aside administrative duties and return to his research work in geophysics.

On leaving the Lake Survey Dr. Woodward joined the United States Transit of Venus Commission as assistant astronomer and was associated with it for two years, 1882 to 1884, with Professors Asaph Hall and William Harkness. During the transit of 1882 he was with the observing field party under Professor Hall at San Antonio, Texas. There were eight field parties, four in the United States and four in foreign countries, and each party took a number of photographs during the transit of Venus across the sun's disk. These photographs were deposited with the Naval Observatory, where they were carefully measured, and the data of measurement were used in computations to obtain a more nearly correct value of parallax than had hitherto been possible. An immense amount of labor was put into this task, but, as a result of improvements in methods along other lines, the parallax values thus derived were superseded before the final report was completed and it was accordingly not issued.

The twelve years thus spent by Dr. Woodward in geodetic and astronomic work of the highest precision, but always with an immediate practical bearing, trained him to an engineer's sense of values and proportion. Whenever a problem arose he instinctively sought not only the means of solving it, but also the most direct method that would give results of the desired degree of accuracy with the least expenditure of energy. We shall now see how in the next decade he utilized this training to the utmost and made to geology and geodesy remarkable contributions that would otherwise have been impossible. This same resourceful attitude of mind also makes for success in administrative work. It was inevitable, therefore, that in later years Dr. Woodward's balanced judgment should be sought on all sorts of technical and scientific problems, and that he should be drawn eventually into executive work.

From the Naval Observatory Dr. Woodward was called in 1884 to the United States Geological Survey as astronomer, where he soon became geographer, and then chief geographer in charge of the Division of Mathematics. The Geological Survey was at this time still young and had much to learn in all its branches; but its members were men—like Gilbert, Dutton,

King, and Chamberlin—who, as masters in geology, with a virgin country to investigate, were full of enthusiasm and eager to accomplish much. It was a congenial atmosphere to enter, and during the next six years Dr. Woodward wrote his most important scientific papers.

His tasks on the Geological Survey were of two kinds, namely, those arising from the topographic branch in connection with field methods and map-work, and the investigation of problems of a geologic nature, but involving the behavior of the earth as a whole. To the first group of tasks Dr. Woodward had long been accustomed. They included latitude and longitude determinations at critical stations in different States, problems on map projection, and the testing of field methods. He prepared a set of formulas and tables to facilitate the construction and use of maps. These tables, in amplified form, are still in use at the Geological Survey and constitute the basis for its topographic sheets. He prepared sets of instructions on methods best adapted for field use in primary and secondary triangulation. With the aid of several members of his division, he set up a small observatory for use in testing field instruments and in giving instruction to topographers in field topographic methods, especially latitude and longitude determinations. As a result of this work, the field methods were put on a practical engineering basis, thus securing the desired degree of accuracy with the minimum expenditure of funds and energy.

Geology at this time was moving forward rapidly, and the geologists of the Survey naturally turned to Dr. Woodward for assistance in problems of a mathematical physical nature. G. K. Gilbert found, for example, that within the area covered originally by Lake Bonneville the shoreline observed on an island in the central part of the lake was 129 feet higher than the strandline at its margin. The question arose, Was this difference in elevation due to the disappearance of the water? T. C. Chamberlin wished also to know to what extent the form and position of the sealevel may have been modified by the attraction of the Pleistocene ice-cap. A general solution to these two problems was given by Dr. Woodward in Bulletin of the U. S. Geological Survey No. 48 on the "Form and position of sea-

level, with special reference to its dependence on superficial masses symmetrically disposed about a normal to the earth's surface." This problem was one requiring for its solution mathematical work of the highest order and, in addition, the experience of the engineer, so to shape his formulas that they could be applied directly by the computer. The problem, as Dr. Woodward attacked it, became that of the deformation of the terrestrial geoid (as expressed by the surface of the sea) by the attraction of an ice-cap or other mass. It meant the investigation, by means of the potential theory, of the change in the equipotential surface of a large lake or sea as a result of the presence of an ice-cap, or of a body of water, or of a continental mass. The new set of formulas developed by this investigation sufficed to answer not only the questions of Gilbert and Chamberlin, but also other related questions concerning the distribution of density in the earth's mass and regarding the effect of continental masses on sealevel. In an historical note appended to this paper, Dr. Woodward analyzes the work of Croll, Archdeacon Pratt, D. D. Heath, and Sir William Thomson on this subject and shows the scope of each treatment. The work of these mathematicians demonstrated clearly the limitations that arise when the analyst is satisfied with a general solution to a problem, but fails to obtain a solution that is suitable for computation purposes. On this point Dr. Woodward insisted repeatedly. Thus, in his papers on the free cooling and on the conditioned cooling of a homogeneous sphere, he "sought to express the solutions in such terms that the computer can, without undue labor, assign the temperature at any point within the sphere for any value of the time, having in mind always an application of the theory to the earth." Again, in his paper on the diffusion of heat in a homogeneous rectangular mass, he refers to the extraordinary studies of Fourier and Poisson and notes that "we must extend their work and adapt it to the practical needs of the computer."

Other problems were referred to Dr. Woodward while he remained with the Geological Survey. Thus Mr. Gilbert, in the course of his studies on the moon's face, wished to ascertain the average angle of incidence of meteors and moonlets impinging on the moon. Mr. Gilbert favored the meteor impact theory for

the formation of the moon's craters and sought an explanation for the absence of elliptically shaped craters, which one might expect to find if the meteorites struck the moon at angles other than about normal to its surface. Dr. Woodward solved the general problem of the frequency of the different angles of incidence and showed how they vary with the assumptions made.

For the Division of Chemistry he studied the ratios of the weights of the chemical elements and made least-square adjustments involving the solution of 30 simultaneous equations.

From the geologist's viewpoint, Dr. Woodward's most important contributions of this period are his papers on the free and conditioned cooling of a homogeneous sphere and his application of the results presented therein to the secular cooling of the earth and to its age. Geological thought with respect to the age of the earth had been dominated for a generation by the conclusions of Lord Kelvin, who in 1862 and again in 1883 stated, after a detailed mathematical investigation of the subject, "We must allow very wide limits in such an estimate as I have attempted to make; but I think we may with much probability say that the consolidation can not have taken place less than 20,000,000 years ago, or we should have more underground heat than we actually have, nor more than 400,000,000 years ago, or we should not have so much as the least observed underground increment of temperature; that is to say, that Leibnitz's epoch of emergence of the *consistentior status* was probably between those dates." Geologists accepted this statement by Lord Kelvin because his arguments, so far as they could follow them, seemed sound. Nevertheless, although they could not refute his statements, they found it difficult to reconcile them with a mass of geological evidence. Dr. Woodward undertook to reexamine the entire problem, and carried the solution much further than had Lord Kelvin or his predecessors.

The problem of a cooling sphere was not new.

"It was very thoroughly discussed in its purely mathematical features by Fourier and Poisson, the pioneers in the theory of heat, and has been much studied by mathematicians of later date. Able and elaborate as their work is, however, it is not well adapted to the needs of practical application; it does not enable one to trace readily and accurately all the phe-

nomena of cooling throughout their entire history. My investigations of this problem were necessarily made partly with a view to supplying the defect just named."

In his paper Dr. Woodward gives the data necessary for calculation of the secular cooling of the earth; the age of the earth derivable from such data; the distribution of the isogeotherms; the rate of increase of underground temperature and its variation with the time; the radial and volume contraction; the stratum of no strain.

He summarized his conclusions in an address in 1889 as vice-president of the Section of Mathematics and Astronomy of the American Association for the Advancement of Science. After presenting an excellent digest of the work of his predecessors, Dr. Woodward remarks that the conclusions of Lord Kelvin are very important if true.

"But what are the probabilities? Having been at some pains to look into this matter, I feel bound to state that, although the hypothesis appears to be the best which can be formulated at present, the odds are against its correctness. Its weak links are the unverified assumptions of an initial uniform temperature and a constant diffusivity. Very likely these are approximations, but of what order we can not decide. Furthermore, if we accept the hypothesis, the odds appear to be against the present attainment of trustworthy numerical results, since the data for calculations, obtained mostly from observations on continental areas, are far too meager to give satisfactory average values for the entire mass of the earth. In short, this phase of the case seems to stand where it did twenty years ago, when Huxley warned us that the perfection of our mathematical mill is no guaranty of the quality of the grist, adding that as the grandest mill will not extract wheat flour from peas-cods, so pages of formulæ will not get a definite result from loose data."

This statement by Dr. Woodward, together with his published re-treatment of the entire problem, set the minds of geologists at rest concerning the restrictions imposed by Lord Kelvin. Some years later the generation of heat by transformations in radioactive substances within the earth's crust was discovered as a factor tending to retard the rate of cooling of the earth and to increase the limits set by Lord Kelvin.

In this same vice-presidential address Dr. Woodward summarized also the opinions regarding the contractional theory of

the earth and the theory of isostasy which had been propounded only a few months before by Dutton, and he inferred "that isostasy is competent only on the supposition that it is kept in action by some other cause tending constantly to disturb the equilibrium which would otherwise result. Such a cause is found in secular contraction and it is not improbable that these two seemingly divergent theories are really supplementary." With reference to the solidity or the liquidity of the earth's interior he notes that "the difficulties appear to be due principally to our profound ignorance of the properties of matter subject to the joint action of great pressure and great heat. It is not clear how our knowledge is to be improved without resort to experiments of a scale in some degree comparable with the facts to be explained." Dr. Woodward was a firm believer in experiment. In his own words, "The price of progress, like that of liberty, is eternal vigilance. One must be ever active, ever patiently persistent, proving all things and holding fast to that which is good."

Dr. Woodward was also interested in the possible laws of arrangement of density in the earth's mass under the assumption that the density increases continually from the surface toward the center. Realizing the dependence of the arrangement of density on certain known properties of the earth, such as its mean density, its surface shape and surface density, and its constant of precession, he sought to determine the most probable law of increase, but found that the available data were not adequate for the purpose.

In 1890 Dr. Woodward resigned from the Geological Survey to accept a position with the United States Coast and Geodetic Survey. Major J. W. Powell, in his Report of the Director of the Geological Survey for 1889-1890, states that Dr. Woodward's "resignation was accepted with regret, as this Survey can ill afford to lose his rare ability for mathematical research. Since his first association with the Survey, in 1884, he has not only supervised the computations made in connection with the triangulation and astronomic determinations, conducted the computation of a series of tables for the use of the Topographic Branch, and given aid to geologists having occasion to read their data by mathematical methods, but he also made important additions to

geologic science by discussing and advancing, on several lines, the theories of terrestrial physics. His discussion of the form and position of sealevel of the earth as dependent on superficial distribution of matter yielded formulas and numerical results of importance in the discussion of the physical results of the filling and emptying of the Bonneville Basin, of the formation and dissolution of the northern ice-sheet, and of all other phenomena involving the influence of superficial masses on the geoid. His discussion of the problem of a heated sphere, such as is presented by the earth, and of the contraction resulting from its slow cooling, is especially valuable for its indication of the additional work needed for the determination of the constants involved, and for the illustration of the dependence of the mathematical results upon important postulates, the possibility of whose future demonstration is not apparent. He has also studied and discussed the distribution of density and pressure within the earth and made a preliminary study of the stresses and strains involved in the deformation of the earth's crust within the limits of elasticity."

Dr. Woodward was called upon by the Coast and Geodetic Survey, because of his long experience in precise triangulation, "to devise means of testing in the most thorough way practicable the efficiency of the various forms of base apparatus used by the Survey, especially the efficiency of long steel tapes or wires." To this congenial task he applied himself with pleasure and enthusiasm. He devised the iced bar apparatus for measuring base lines and for calibrating steel tapes. His method for calibrating steel tapes was adopted by the Coast and Geodetic Survey and remains still the standard method. He was also the first to measure primary base lines with long steel tapes, and to prove that these tapes furnish the required degree of accuracy, namely, one part in one million. This work was of fundamental importance to geodesy and not only resulted in the saving of much money and time in field-work, but placed the primary triangulation work of the Coast and Geodetic Survey on a higher plane than had been theretofore possible. He insisted, moreover, that, in the field, measurements only of the kind and number be taken that are necessary to insure the desired degree of

accuracy. His influence led Hayford some years later to revise and standardize the field and computational methods of the Coast and Geodetic Survey, thereby greatly increasing the speed of the work, lowering the costs, and placing the results on a uniformly high level of known precision. In this respect the Coast and Geodetic Survey set the example for other surveys the world over.

Maps constitute a very important item in geology, and for this contribution alone geology owes much to Dr. Woodward.

While at the Coast and Geodetic Survey Dr. Woodward prepared for the Smithsonian Institution the Smithsonian Geographical Tables. In the introductory part (93 pages) of this volume useful formulas are given for the geographer and cartographer, the geodesist and astronomer; also a brief statement of the theory of errors is included. The tables cover 180 pages and furnish data of interest to geographers and students of allied sciences. The third edition of these successful Tables was issued in 1906 and was reprinted in 1918.

In 1893 Dr. Woodward was called to Columbia University as Professor of Mechanics and Mathematical Physics. From that time on, his work was that of the teacher and administrator. In 1895 he became dean of the College of Pure Science and was confronted with many problems that required tact and perseverance for their solution. His influence on the student body and on the students directly under him at Columbia was remarkable. He insisted on good work from each student, but he was ever ready to give help where it was needed. His thorough knowledge of mechanics and of the needs of the engineer enabled him to give to his engineering students the kind of information they most needed, and to train them to the attitude of mind essential to successful accomplishment in engineering work.

Intensely interested in the human element, and with a most attractive, genial, and lovable personality, his advice was sought on every side, both by the student body and by the faculty; so that, in spite of the best intentions and very hard work, he could not obtain for himself the desired periods of quiet to pursue his mathematical work, to which he looked ever forward.

He did, however, undertake, with the aid of Professor Wills and Dr. Deimel, an investigation into the possibilities of the double suspension pendulum for the determination of the value of gravity. The advantage of this method over the ordinary method is the ease with which the length of the pendulum can be ascertained; its disadvantage is the uncertainty regarding the elastic behavior of the suspension fibers or ribbons. Dr. Woodward made a thorough mathematical investigation into the subject, but was called to another position before the experimental work had been completed, and he never found an opportunity to finish the task. The records of the investigation, so far as they were carried, are on file in the Physics Department of Columbia University.

His interest was also aroused by A. L. Queneau in the cooling and crystallization of intrusive igneous masses as an example of a special case of the general theory of the cooling of a heated mass (bar or sphere), as developed by Woodward in 1888. Dr. A. C. Lane was the first to treat this problem, but his solution was somewhat different from that of Dr. Woodward. Queneau adopted Woodward's mode of treatment and applied it to a study of certain igneous rocks, arriving at essentially the conclusions reached by Dr. Lane.

On December 13, 1904, Dr. Woodward became president of the Carnegie Institution of Washington, and met the problems of this new kind of research institution with the same good common sense that characterized all his executive work. He analyzed each problem thoroughly, including that of personnel, and insisted that the institution be run on a thoroughly businesslike basis, that it live within its income, and that its money be spent with fair expectation of a good return. He established several departments of the institution and organized the institution about them. Miscellaneous appropriations to aid individual pieces of research were carefully scrutinized and every effort was made to spend the money entrusted to his care wisely and in the interests of science. Dr. Woodward showed keen appreciation of scientific activities outside his own sphere of astronomy and earth science and in his later years was much interested in archæology and in the humanities generally.

He believed that he who thinks clearly writes clearly. "It is so much easier to appear to write well, or even brilliantly, than it is to think clearly, that facile expression is often mistaken for sound thought." He himself wrote exceedingly well and coined many apt and telling phrases.

In his relations with the men of the institution under him he showed the keenest interest and encouraged them wherever possible. His home life was of the ideal kind. Both he and Mrs. Woodward had a genius for hospitality and made each visitor feel at ease. Their home in Washington was ever the meeting place for scientific and other folk, just as it had been at Columbia University.

In the early days of the Carnegie Institution the effort to find and to aid the unusually talented man in his scientific work, in accordance with the expressed wish of the founder, Mr. Carnegie, led to the placing of numerous small grants with men over the country who had special problems to solve. When Dr. Woodward became president he examined carefully into these random grants and found that a surprisingly small number yielded returns to science commensurate with the outlay. He therefore concluded that, so far as possible, the efforts of the Carnegie Institution should be concentrated on projects of large scope, which would probably not otherwise be attempted, and that these efforts for the most part should be carried forward by investigators in the employ of the Institution. The result was greater emphasis on the departments of the Carnegie Institution, such as the Geophysical Laboratory and the Mount Wilson Observatory, and the more intensive development of all these departments. A few research associates who were able to give their entire time to special research problems were also maintained as integral members of the Institution, but the voting of small grants to aid in the solution of specific problems under attack by individuals connected with other institutions was minimized as a policy of the Institution.

At the outbreak of the war Dr. Woodward was much disturbed by the trend of events in Europe, and, when this country entered the war, he was influential in persuading the trustees to offer the services of the entire Institution to the Government.

The war work of the Carnegie Institution covered many fields of activity, from the manufacture of optical glass to military intelligence work, and to it all Dr. Woodward offered the most effective support. He himself was a member of the Naval Consulting Board, as one of the two representatives from the American Mathematical Society to the board.

Dr. Woodward was president of the Carnegie Institution from December 13, 1904, to January 1, 1921, a period of sixteen years. The first president of the Institution, Dr. D. C. Gilman, president emeritus of Johns Hopkins University, had retired from active life, and he undertook the task only temporarily, until the best man for the position could be selected. When Dr. Woodward entered upon his duties the Institution was two years old and had still to establish itself in the minds of the public and to determine on the best policies to follow. It needed at this critical period the mature judgment and experience that Dr. Woodward had acquired in governmental and educational positions; it needed also an enthusiastic and genial personality to carry through successfully the projects that merited encouragement and at the same time to avoid the far greater number of suggested projects which, although urged upon the Carnegie Institution most insistently, really promised little return. On looking back over his years of activity, he states in his last report as president that "probably no other organization in the evolution of learning has been so beset by what Dr. Johnson called the anfractuosities of the human mind as the Carnegie Institution of Washington." But his keen sense of humor carried him over the rough places and his kindly, well-balanced attitude instilled in those with whom he came in contact the feeling of assurance that each proposal would be dealt with fairly and on its merits. Thus it was that the Carnegie Institution grew and flourished and contributed much to the development of science in this country.

Dr. Woodward received many honors from different scientific societies. He was vice-president of the Section of Mathematics and Astronomy of the American Association for the Advancement of Science, in 1889; its treasurer, 1894-1924, and its president in 1900; president of the American Mathematical Society,

1898-1900; of the New York Academy of Sciences, 1900-1902; of the Washington Academy of Sciences, 1915; of the Philosophical Society of Washington, 1910, and of the Literary Society of Washington, 1913-1914. He was a member of the National Academy of Sciences, the Astronomical Society, and the American Physical Society, and fellow of the American Academy of Arts and Sciences and of the American Philosophical Society. He was elected a member of the National Academy of Sciences in 1896. He served as member of its Council from 1909 to 1915 and on the following Academy committees: Weights, Measures, and Coinage (1906-1924); Gould Fund (1915-1924). He was Chairman of the Section of Physics and Engineering (1908-1913); and Chairman of the Special Committee appointed in 1913 to advise the Secretary of Agriculture on the selection of a new Chief for the United States Weather Bureau. He was a member of the Executive Board of the National Research Council, 1918-1920; of the Executive Committee of its Division of Physics, Mathematics, Astronomy, and Geophysics, 1918-1920; and of the Section of Geodesy, of the American Geophysical Union, 1919-1920.

He was associate editor of *Science* from 1884 to 1924, and of the *Annals of Mathematics* from 1888 to 1889. Together with Prof. Mansfield Merriman, he edited a series of mathematical monographs; he himself contributed in 1896 to the series the monograph on the "Theory of Probabilities."

From the universities he received many honorary degrees: University of Michigan, Ph. D. in 1892; LL. D. in 1912; University of Wisconsin, LL. D. in 1904; University of Pennsylvania, Sc. D. in 1905; Columbia, Sc. D. in 1905; Johns Hopkins, LL. D. in 1915. He was a member of the Century Association of New York and of the Cosmos Club in Washington.

In 1876 he married Miss Martha Gretton Bond of Detroit who with three sons survived him.

In addition to his great attainments, Dr. Woodward possessed a simplicity and open friendliness of manner and character that endeared him to young and old alike. It was a privilege to know him, to feel his radiant enthusiasm, and to be uplifted by his hopeful outlook on this world and its many problems.

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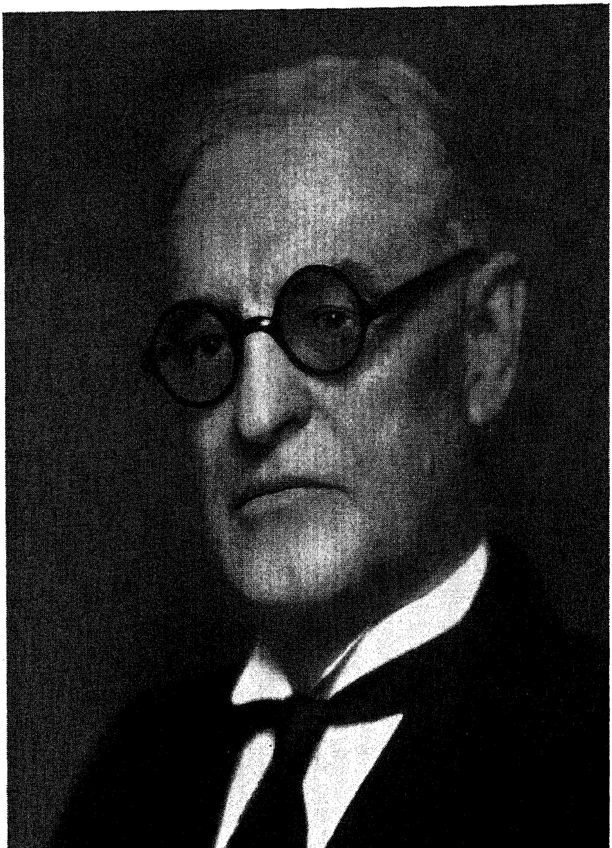

Edwin B. Frost

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OF

EDWIN BRANT FROST

1866–1935

BY

OTTO STRUVE

PRESENTED TO THE ACADEMY AT THE AUTUMN MEETING, 1937

EDWIN BRANT FROST

1866–1935

BY OTTO STRUVE

As I write this memoir of the life of Edwin Brant Frost I am deeply conscious of the fact that the most characteristic feature of his life's work was the international scope of his scientific interests. Mr. Frost was one of the most outstanding representatives of a large group of American scientists who recognized no narrow national barriers in science. Throughout his long and distinguished career he worked for international co-operation in astronomy—his chosen field—and many of the recognized achievements of international meetings and committees owe their success, directly or indirectly, to his efforts.

The disturbing political events of the past few years and months tend only to strengthen our realization of the enormous value of Frost's efforts. Since the beginning of the world war conditions have not been conducive to the development of true international cooperation. There are now fewer men in science, who, like Frost, had an opportunity to study in Europe. Likewise, there have been fewer foreign students in America. Regulations of foreign governments concerning the exportation of currency and political restrictions have, for many years, prevented the normal exchange of fellows, traveling students, and visiting scientists. Even the exchange of books and periodicals is becoming increasingly difficult because of monetary restrictions or because of rapid fluctuations in the foreign exchange.

When Professor George E. Hale organized the *Astrophysical Journal* in 1895, he added to the name of the new periodical the subtitle: *An International Review of Spectroscopy and Astronomical Physics.* In a plan of publication formulated in Berlin it was decided "that five associate editors be chosen to represent Germany, Great Britain, France, Italy and Sweden on the editorial staff, for it was felt from the first that unless the *Journal* were made truly international in character it could not be a success." The original board of editors consisted of

Hale and Keeler. Frost, Ames, Campbell, Crew, and Wadsworth acted as assistant editors. Cornu, Dunér, Huggins, Tacchini, Vogel, Hastings, Michelson, Pickering, Rowland, and C. A. Young were the associate editors. After he was made acting editor in 1902, together with Hale, Frost continued the international policy outlined by the latter, and to this day the *Astrophysical Journal* includes in its list of collaborating editors a group of five distinguished astronomers and physicists of other countries.

When, during the war, Professor Elis Strömgren organized at Copenhagen his international bureau of astronomical information between the belligerent countries, Frost whole-heartedly supported it. When after the end of the great war German and Austrian observatories were unable to pay the subscription price for the *Astrophysical Journal,* Frost devised a plan whereby the *Journal* was sent to them free, payment being deferred until economic conditions in central Europe would improve.

Having learned of the distressing conditions in famine-stricken Russia, Frost organized in the spring of 1922 an informal committee (with the cooperation of Professor Van Biesbroeck and the writer) for the relief of Russian astronomers. This committee collected the considerable sum of $2,556.12 among American astronomers and friends of science, and disbursed the entire amount to the suffering astronomers in Russia, mostly in the form of Mr. Hoover's ARA food packages. Although some efforts had been made earlier to help the astronomers at Pulkovo, the immediate stimulus for the formation of Frost's committee came through a letter to him from the distinguished director (now deceased) of an eastern observatory in Russia. This letter begins as follows: "The severe conditions of famine which now prevail in Russia compel me as director to appeal to you for help for the staff of the observatory . . . " Touching are some of the replies acknowledging the receipt of the packages: "We Russian astronomers during many years have been separated from the whole civilized world, but now we feel our bonds renewed with the men of learning and the progress of universal science." It was indeed a satisfaction for Mr. Frost

to learn that a ten-dollar food package had carried two persons through a month.

In his autobiography "An Astronomer's Life" Mr. Frost has given an excellent account of his life. He was born on July 14, 1866, at Brattleboro, Vermont, the descendant of one Edmund Frost, who was born about 1600 in England, and came to America in 1635 in order "to escape the more savage oppression of England." Edwin Brant's father—Carlton Pennington Frost—was a professor and dean of the medical school at Dartmouth College. Edwin Brant Frost spent many years of his early life at Hanover. In 1882—about a month before his sixteenth birthday—he passed the entrance examinations at Dartmouth College. In his senior year he became interested in astronomy. Professor C. A. Young had moved to Princeton in 1877, but Frost knew him and his family quite intimately and Fred, the younger son of Professor Young, had been Edwin Brant's chief playmate. In August, 1885, a nova appeared in the great spiral nebula of Andromeda. It was then believed that "we might be observing the sudden transformation of the nebula into a star along the lines of the theory of Laplace." Frost was greatly interested in the new phenomenon and chose it as the topic of his senior class oration. He graduated in 1886, with a speech on the influence of astronomy upon literature.

After graduation, Frost enrolled as a post-graduate in the department of chemistry, and almost at once was appointed assistant under Professor Bartlett. For a short time he taught school at Hancock, New Hampshire. Early in 1887 he accepted an invitation from Professor C. A. Young and went to Princeton to take a practical course in astronomy, being a guest in the Young household. In the fall of 1887, Frost was appointed instructor in physics and astronomy at Dartmouth College. He was then only 21 years old. In the summer of 1888 he assisted Professor Young in reading the proofs of the latter's textbook on *General Astronomy*. This excellent book made a great impression upon the young astronomer. With his brilliant memory he was able to retain until his death an almost photographic mental record of nearly every page! Even after total blindness had

prevented him from consulting this book, he could unerringly point out the page and paragraph when he wanted someone to read a particular passage to him.

In 1890 Frost took leave of absence from Dartmouth and went first to England where, among others, he met Sir William and Lady Huggins. At Greenwich he met the chief assistant, H. H. Turner—later Savilian Professor of Astronomy at Oxford—who remained one of his life-long friends. After a short stay at Strassbourg, where he attended lectures by Kohlrausch, Wiener, Becker and others, and where he became acquainted with other graduate students and assistants: Peter Lebedeff of Russia, H. Kobold (now in Kiel), J. Halm (later astronomer at the Cape of Good Hope), Frost went to Potsdam to work under Vogel. In 1891 he was appointed assistant at Potsdam under Scheiner and Vogel. Nova Aurigae provided much excitement that year, and Vogel assigned to Frost the task of observing its spectrum. It was through this somewhat accidental occurrence that Frost embarked upon the study of stellar spectra, which later became his special field of research.

Scheiner had just published an important new book on the "*Spectral Analyse der Gestirne,*" and Frost decided to translate it into English. This translation, containing many additions and revisions by the translator, was printed by Ginn and Company in 1898. It remained the standard textbook of astrophysics in the English language for almost twenty years.

In the autumn of 1892 Frost returned to Dartmouth College as assistant professor of astronomy. In the same year he met Dr. George E. Hale at the Rochester meeting of the American Association for the Advancement of Science. It was at this meeting that Hale learned from Alvan G. Clark that two excellent 40-inch discs cast by Mantois of Paris were being offered for sale. Frost probably did not then realize that he would be custodian of these discs for more than a quarter of a century.

In 1897 Frost attended the conference of astronomers held in connection with the dedication of the Yerkes Observatory at Williams Bay, Wisconsin. The following April Professor Hale invited him to become professor of astrophysics at the Yerkes Observatory of the University of Chicago.

When Hale organized the famous expedition from the Yerkes Observatory to Pasadena, which formed the beginning of the Mount Wilson Observatory, Frost remained at Williams Bay as acting director. In 1905, upon Dr. Hale's resignation, he was appointed director—a position which he held until his retirement in 1932.

In the night of December 15, 1915, Frost lost the use of his right eye. He writes in his autobiography: ". . . I was working alone at the forty-inch telescope, photographing the spectrum of a rather faint star, by name 20 Persei. I had difficulty in seeing the divisions of the circle and in guiding after I had brought the star into proper position upon the slit of the spectroscope. I carried on the exposure until an assistant arrived and then found that vision in the right eye was greatly reduced. I had my own fear of what was the trouble, because my mother had suffered from a detached retina. My fears were unfortunately well founded." Frost retained the use of his left eye and was able to read with it. In 1921 he lost the use of this second eye. I clearly remember how, on October 10, of that year, Mr. Frost met me at the station in Williams Bay when I arrived from Constantinople. With Professor Van Biesbroeck to help him overcome the restrictions of his vision (due then mostly to nearsightedness in the remaining eye) he drove his own car to the Observatory, where he introduced me to my duties as his assistant. A few nights later I was observing at the forty-inch refractor, and forgot to mark the spectrograms for identification. The next morning Mr. Frost came to my office and offered to help me identify the stars from their spectra. I lined them up for him in the Hartmann spectrocomparator, but, to his surprise, he could not see the lines. It was found later that a hemorrhage had occurred in the good eye; and a rapidly forming cataract—which never ripened and thus could not be removed—soon completely extinguished his vision.

The tragedy of a great astronomer becoming blind cannot adequately be described. Mr. Frost gave an example to everyone who knew him of courage and cheerfulness. He adjusted himself to his lack of vision, and continued, for eleven years, to direct the Yerkes Observatory and to edit the *Astrophysical*

Journal. One of his biographers fittingly wrote of him: "This cruel malady—and what could be harsher than blindness to an astronomer?—deafness to a musician perhaps—brought out such magnificent traits of character and aroused in those who saw him go about his ways in cheerful mien, such quick sympathy and profound admiration that the influence of his dark years upon his fellow-men was perhaps even greater than those devoted actively to research." Probably no other scientist has ever been admired and loved so much by those who knew him or knew of him. Children used to gather around him and hear his stories about the stars, or the birds which he could imitate to perfection, or the trees which he had planted in the park surrounding the Observatory. Grown-up people of all classes of life listened reverently to him when, on a Saturday afternoon, he explained to them the operation of the great telescope. At the opening of the Century of Progress Exposition in Chicago in 1933, when the light of Arcturus was used to turn on the illumination of the Fair grounds on the shore of Lake Michigan, Mr. Frost gave the principal address which was heard by many thousands of visitors in Chicago and by millions of listeners over the radio. A well-known astronomer dedicated to him a large volume of astronomical research, in the following words:

"TO
EDWIN BRANT FROST
MY FIRST TEACHER OF ASTRONOMY
THIS VOLUME
IS GRATEFULLY INSCRIBED
IN THAT NIGHT INTO WHICH HE HAS FOLLOWED
GREAT GALILEO MAY HE STILL SEE WITH THE
EYES OF HIS DEVOTED AND REVERING STUDENTS."

Frost's astronomical work began in 1889 at the Shattuck Observatory of Dartmouth College. He observed sun spots, comets, occultations, and in 1891 he computed the orbit of Comet 1890 IV (Zona). In 1892 he published an important paper on the thermal absorption in the solar atmosphere, which was inspired by H. C. Vogel at the Potsdam Observatory. In later years Frost retained his interest for solar investigations: he was

an ardent observer of the flash spectrum during solar eclipses. At the eclipse of May 28, 1900, he obtained a series of excellent exposures of the flash spectrum and his measurements of the wave-lengths and identifications of the lines were among the best at that time, and formed the basis for later work by other observers.

The great majority of Frost's scientific contributions deal with the spectra of the stars and, in particular, with the determination of their motions in the line of sight.

The original equipment of the Yerkes 40-inch refractor included a stellar spectrograph designed by Professor Keeler of the Allegheny Observatory and constructed by Mr. Brashear of Pittsburgh. This instrument was described by Professor Hale and Mr. Ellerman, and was used by them to great advantage for a study of the spectra of very red stars. However, the mechanical parts were not sufficiently stable and free of flexure to permit its use for the determination of radial velocities. Accordingly, Frost decided to build a new modern spectograph intended primarily for radial-velocity work. In 1899 Miss Catherine W. Bruce made a gift to the Yerkes Observatory of $2300 for the new instrument, and in January, 1902, Frost was able to publish in the *Astrophysical Journal* a complete description of the Bruce spectrograph, together with some preliminary measurements of stellar motions.

The Bruce spectrograph remained Frost's principal instrument, and with it he accumulated an enormous amount of material on the spectra of the hottest stars, spectroscopic binaries, novae and variable stars. It is of interest in this connection that the Bruce spectrograph has for over thirty-five years been the principal spectrographic instrument of the Yerkes Observatory. Without major changes it has served two generations of astronomers, and even today it continues to produce results which are equal to those obtained elsewhere. With a short-focus camera designed by Dr. G. W. Moffitt and with one of the original prisms made of Mantois glass, recently refigured by Bausch and Lomb, we are able to photograph the spectra of stars of the eleventh photographic magnitude in a few hours, with a dispersion of about 120 A/mm. at λ 4500. The defini-

tion given by the single prism is excellent: with a longer camera it has recorded a vast number of spectral lines never before observed with any instrument.

Incidentally, the old Brashear spectrograph of the 40-inch telescope was transferred by Frost to the 12-inch refractor, where it was used for many years in the observation of prominences. A few years ago it was again used for stellar work by Messrs. Elvey and Keenan, who obtained with it the total intensities of Hα in several stars. Quite recently this old spectrograph, equipped with a single prism and with a new short camera, has again been transferred to the 40-inch, where Dr. G. P. Kuiper uses it for the classification of the spectra of stars as faint as magnitude 13.

Frost's first work with the Bruce spectrograph dealt with the motions of the helium stars. These stars had been somewhat neglected by other observers, partly because their lines are often ill-defined and partly because the laboratory wave-lengths of many of the lines were not adequately known. In collaboration with Dr. W. S. Adams, Frost overcame these difficulties and in 1904 there appeared in the *Publications of the Yerkes Observatory* a paper, under joint authorship, on the "Radial Velocities of Twenty Stars having Spectra of the Orion Type." A careful investigation of the systematic errors of the instrument preceded the work, and the precision obtained for the twenty stars was very gratifying. Although the number of stars was not sufficient to make a solution for the solar motion, "the distribution of positive and negative velocities shows clearly the direction of the motion of the sun in space." The mean motions of the helium stars were found to be surprisingly small, only 7.0 km/sec as the mean of the twenty radial velocities corrected for solar motion. It was already known that the average velocities of the cooler stars were considerably larger. Thus, Frost and Adams brought out the significant fact that the average motions of the stars were not the same for all spectral types. This result has been of fundamental importance in all later investigations concerning the dynamics of the stellar system.

But the most significant result was stated by the authors in the following short sentence: ". . . if the sign be regarded, the mean becomes +4.6 km/sec." In other words, after the component of the solar motion had been subtracted from the measured velocities, the mean velocity with regard to the sign was not zero or close to zero, but was of the same order of magnitude as the mean velocity taken without regard to sign. The authors had thus, for the first time, recorded the famous K-effect in the motions of the helium stars, which, literally interpreted, means that the system of B-type stars, as a whole, expands with a velocity of 4.6 km/sec.

In 1910 Frost, in collaboration with J. C. Kapteyn, returned to the question of the mean motion of the helium stars. After having discussed the solar motion from the large amount of radial velocity material collected by Frost, the authors remark: ". . . meanwhile our numbers bring out a somewhat unexpected fact, namely that the velocity of the sun relative to the stars near the apex is found to be very different from that relative to the stars near the antapex. To show this more clearly, a separate solution was made for the stars for which $\lambda < 90°$ and for which $\lambda > 90°$. We thus find:

Near apex $v = -18.38 \pm 1.40$ km, from 32 stars
Near antapex $v = -28.38 \pm 1.36$ km, from 29 stars
Simple mean $v = -23.38$ km, from 61 stars

The difference is very considerable and cannot well be attributed to accidental error alone."

This peculiar phenomenon of expansion of the system of helium stars has been the subject of many later investigations, and even today it has only been partly explained. Kapteyn and Frost were aware that "a constant error, depending on instrumental and personal influences and on errors in the assumed wave-lengths of the lines both in the star spectrum and in the comparison spectrum such that the positive velocities would result too great, might of course explain the difference." To test this possible explanation Frost devoted much time and energy to the determination of the wave-lengths and to the elimination of systematic errors in radial velocities. But the

K-effect persisted. It is of historic interest that Kapteyn and Frost concluded that "the most plausible [explanation] would seem to be that either the stars near the apex, or those near the antapex, or both, belong in unequal numbers to the two great star-streams." A somewhat similar explanation, in terms of systematic motions among the brighter helium stars, was recently proposed by Paskett and Pearce, at Victoria.

The final radial velocity results obtained at the Yerkes Observatory were published by Frost, Barrett and Struve in the *Astrophysical Journal,* 64, 1, 1926, and in the *Publications of the Yerkes Observatory,* Volume VII, Part 1, 1929. The former contains the velocities of 368 B-type stars and the latter those of 500 A-type stars.

One of the earliest results of Frost's work on radial velocities was the discovery of a surprising number of new spectroscopic binaries. The pages of the *Astrophysical Journal* were virtually swamped with announcements of new binaries. Their number grew so rapidly that Frost was, at times, concerned over the question whether enough stars of constant velocity would be left to provide sufficient material for a study of the motions of the system of helium stars. He and his associates, therefore, began the laborious task of determining the orbits of some of these binaries.

On May 14, 1902, Frost made an important discovery, although at the time he himself probably did not realize how deeply it would affect our knowledge of the stars. On that particular night he took two spectrograms of the bright B-type star β Cephei. During the winter of 1901-1902 he had measured eleven spectrograms of this star and had found it to possess a variable velocity. He states in his first announcement concerning β Cephei: "We had assumed from the first plates that the period would be rather long, but a suspicion to the contrary led me to take two plates on the night of May 14, and during the interval of five and one-half hours the velocity changed 14 km, or nearly half of the whole range so far observed." Such rapid variations in radial velocity were quite unheard of in 1902, and Frost diligently continued his observations. Four years later he announced that the period is $4^h34^m11^s$, and that the velocity

curve is nearly symmetrical, with a range of 34 km/sec. The shortest period of any spectroscopic binary previously known was 1.45 days in the case of μ Scorpii and V Puppis. Frost noticed that the radius of the orbit of β Cephei, computed from the velocity curve, would be inconceivably small—only 45,000 kms—and he suggested that the binary hypothesis could only be true if the inclination of the orbit were close to 0°. He says: ". . . if the radius of the orbit of the brighter star is assumed for the moment to be the same as that found by Vogel for Algol (1.6 million kms), then the inclination of the plane would lack only about 1½° of 90°; and the observed projected velocity would have to be increased nearly forty-fold, yielding an actual velocity of over 600 km/sec."

β Cephei was not then known to vary in light, and this was believed to support the hypothesis of a small inclination. Frost was therefore immensely interested when, in 1913, Guthnick announced that the light of β Cephei varies with an amplitude of 0.05 magnitude, the period of this variation being identical with that found by Frost. However, the light curve did not resemble those of ordinary eclipsing variables. Furthermore, it seemed inconceivable that the small range in radial velocity could at all be reconciled with an eclipse. The problem of β Cephei remained unsolved for many years.

Frost clearly understood that in order to explain the apparent contradictions in β Cephei, more observational material on other stars was required, and he started systematic observations for the detection of other similar objects. Every star known to have a variable radial velocity, for which no period had been found, was placed on the observing program and long series of spectrograms were obtained on a few single nights. Most of these cases yielded only constant velocities, but a small number of stars were gradually discovered which seemed to resemble β Cephei. Other observatories added to the list, and by 1921 the number was sufficient for a preliminary discussion. This discussion Mr. Frost assigned to me, a few weeks after my arrival at the Yerkes Observatory. For spectroscopic binaries of types O, B and A, the amplitude of the velocity curve increases, on the average, rapidly with decreasing period, in accord-

ance with Kepler's third law. But this increase continues only until a period of about 1.3 days is reached. For still shorter periods the amplitude again decreases. This can mean only one thing: β Cephei and all similar stars are not true binaries. They resemble more closely the Cepheid variables, but differ from them by the small amplitude of their light variations. Many representatives of this group display irregularities in their light curves as well as in their velocity curves. The entire group has often been designated as the β Canis Majoris stars, and Henroteau, who, I believe, first introduced this term, counted β Cephei among them. The name is, however, historically incorrect, and scientifically misleading. β Cephei was, without doubt, the first and most typical representative of the mysterious group of quasi-Cepheids of early type and of very short period. While irregularities are present both in its velocity curve and light curve, it promises to give valuable results to the observer who can combine photoelectric observations of the light with accurate measurements of the radial velocity. We know surprisingly little concerning these stars, and Frost's work on β Cephei will become increasingly appreciated as we begin to unravel their secrets.

In 1917 Frost started a long series of observations of the peculiar spectroscopic binary and eclipsing variable ε Aurigae. Its period had been found by Ludendorff to be 27 years, and it was important to determine the spectrographic orbit as well as to elucidate some very strange phenomena reported by the Potsdam observers during the eclipse of 1902. This work was continued by the present writer and by Elvey, and the results of the Yerkes Observatory investigations were published by Frost, Struve and Elvey in the *Publications of the Yerkes Observatory*, Volume 7, Part 2, 1932. The spectrographic observations made during the eclipse revealed an unsymmetrical broadening of the lines towards the red during the partial phase preceding total eclipse and towards the violet following total eclipse. The authors interpreted this phenomenon as being produced by the absorption within the rotating outer layers of the eclipsing star. Almost simultaneously Guthnick advanced the same hypothesis in the case of ζ Aurigae. These

two stars have given the first indication of the existence in certain super-giants of tenuous atmospheres (or reversing layers) extending to heights of more than one astronomical unit. In most ordinary stars the height of the reversing layer is of the order of a few hundred kilometers. The complete interpretation of the system of ε Aurigae was not possible in 1932, and there remained a serious discrepancy between the evidence furnished by the velocity curve and that furnished by the light curve. The latter suggests that the eclipse of the bright F star is total. Consequently, at constant minimum we should observe the spectrum of the eclipsing star alone. Yet, the spectral lines at minimum are, with very few exceptions, stronger replicas of the normal F-star lines. On the other hand, at maximum separation of lines, around 1925, only the normal F star was visible; the other set of lines was completely absent. Recent progress in the study of ζ Aurigae and VV Cephei has paved the way for the explanation of this apparent paradox. It is expected that the riddle of ε Aurigae will soon be solved—thus bringing to a conclusion a series of investigations inaugurated by Frost twenty years ago.

From the beginning of his work with the Bruce spectrograph Mr. Frost was interested in the problem of systematic errors in radial velocities; his efforts, over a period of thirty years, have profoundly influenced the methods of radial-velocity observers. In 1902 he sent a circular letter to some of the world's leading observatories inviting them to cooperate in the observation of a selected list of ten "fundamental velocity stars." As Frost remarked, "the history of progress in other fundamental measurements would suggest that systematic differences between the results at different observatories are likely to become more apparent as the accuracy of the determinations increases." The observations of planets or of the moon give, of course, an excellent check for each individual observatory, but since the latter have extended surfaces they illuminate the slit of the spectrograph uniformly. Stars which are more nearly point sources must be "guided" on the slit. Lack of exact centering of the star image on the slit caused, for example, by atmospheric dispersion or by optical defects in the guiding system, introduce a

troublesome systematic error which is not present in planetary observations. Furthermore it is important to have the test objects well distributed over the sky so that observations can be made at all times. This would not always be true in the case of planets.

As a result of Frost's continuous efforts a large mass of data on the standard velocity stars has been accumulated at various observatories. In 1925 the International Astronomical Union appointed a sub-committee of the commission on radial velocities for the selection of a new list of standard velocity stars. Frost was the chairman of this committee. The final list published in the *Transactions of the International Astronomical Union*, Vol. III, 1928, contains 28 stars in both hemispheres; this list includes most of Frost's original standard stars. The adopted standard velocities are based upon several thousand individual observations made at twelve observatories. They are, to date, the most precise set of radial velocities known. The probable error for each star is of the order of ± 0.1 km/sec.

The final results of the Yerkes observations of standard velocity stars were used for the derivation of a set of stellar wave-lengths for use with dispersions of around 10 A/mm at λ 4500. These were published as a "List of Recommended Wave-lengths for Three-Prism Dispersion" in the *Transactions of the International Astronomical Union*, Vol. V, 1935.

After his appointment as director of the Yerkes Observatory, Mr. Frost was obliged to devote a large part of his time to administrative duties. But he took a keen interest in the work of his associates. Of the original staff, after Hale, Ellerman, Adams, Ritchey and Pease had gone to Mount Wilson, Burnham, Barnard and Barrett remained permanently with Frost at Yerkes. Schlesinger soon left to become the director of the Allegheny Observatory. Fox remained until 1909 when he was appointed director of the Dearborn Observatory. Of Mr. Frost's later associates we must mention Mitchell, Slocum, Lee, Van Biesbroeck and Ross. Mr. Frost was especially fortunate in having with him as scientific secretary of the Observatory a close friend and loyal collaborator—Professor Storrs B. Barrett.

Mr. Frost had many honors. The University of Cambridge,

England, conferred upon him the honorary degree of D. Sc. in 1912. He also had a D. Sc. from Dartmouth in 1911. He was elected a member of the National Academy of Sciences in 1908; of the American Philosophical Society in 1909; of the American Academy of Arts and Sciences in 1913; of the Washington Academy of Sciences in 1915. He was made a foreign associate of the Royal Astronomical Society in 1908. He held honorary memberships in the Societa degli Spettroscopisti Italiani; the Astronomical Society of Mexico, the Royal Astronomical Society of Canada, and the Russian Astronomical Society.

He was married in 1896 to Miss Mary E. Hazard of Boston. His family life was a happy one, and he has set a monument to it in his book "An Astronomer's Life," which is dedicated to Mrs. Frost. Touching are the pages in which he describes her faithful help in preparing the manuscript for the book, in assisting him with his lectures and in smoothing over the many difficulties which came to him with his blindness. Three children survive him: Miss Katharine Brant Frost is in business in Chicago; Mr. Frederick H. Frost, a paleobotanist, is assistant superintendent of the Warren Paper Company in Portland, Maine; and Mr. Benjamin DuBois Frost is a business man in New York City.

Mr. Frost died on May 14, 1935, after an operation at Billings Hospital of the University of Chicago.

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EDWIN BRANT FROST

List of Abbreviations

A. and Ap. = Astronomy and Astrophysics.
A.J. = Astronomical Journal.
A.N. = Astronomische Nachrichten.
Ap.J. = Astrophysical Journal
J.Can.R.A.S. = Journal of the Royal Astronomical Society of Canada.
Mem. Nat. Ac. Sc. = Memoirs of the National Academy of Sciences.
Mem Spettr. It. = Memorie della Societa degli Spettroscopisti Italiani.
N.S. = New Series.
P A. = Popular Astronomy.
Proc. A.A.A S. = Proceedings of the American Association for the Advancement of Science.
Proc. Am. Phil. Soc. = Proceedings of the American Philosophical Society.
Proc. Nat. Ac. Sc. = Proceedings of the National Academy of Sciences.
Publ. = Publication.
Publ. Am. Astr. Soc. = Publications of the American Astronomical Society.
Publ. A.S.P. = Publications of the Astronomical Society of the Pacific.
Publ. As. and Ap. Soc. Am. = Publications Astronomical and Astrophysical Society of America.
Sid. Mess. = Sidereal Messenger.

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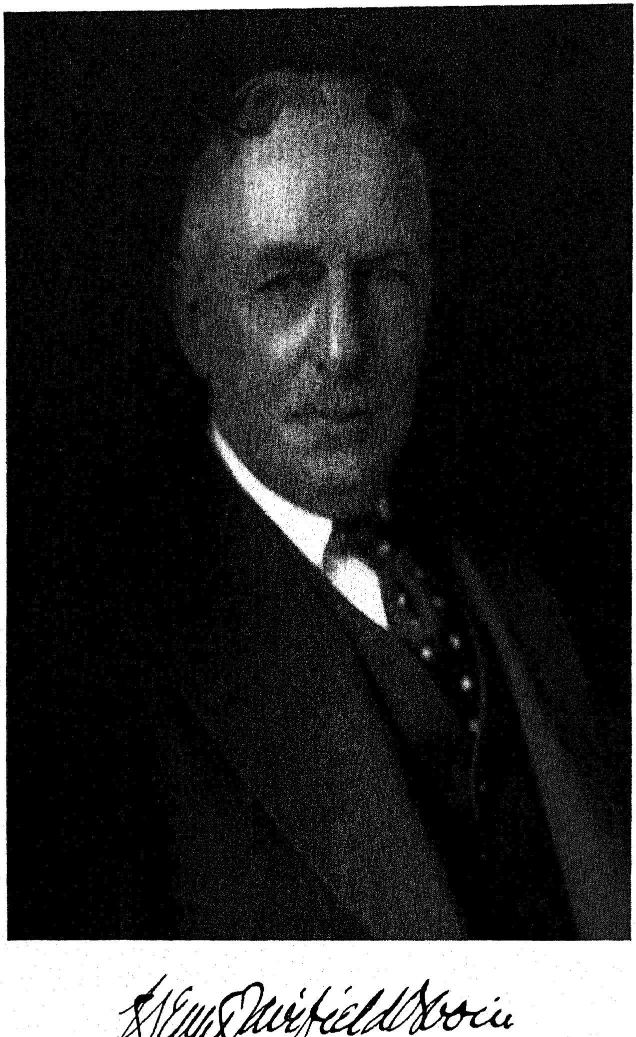

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OF

HENRY FAIRFIELD OSBORN

1857-1935

BY

WILLIAM K. GREGORY

PRESENTED TO THE ACADEMY AT THE AUTUMN MEETING, 1937

HENRY FAIRFIELD OSBORN

1857–1935

BY WILLIAM K. GREGORY

Henry Fairfield Osborn was born at Fairfield, Connecticut, August 8, 1857, the second child and eldest son of William Henry Osborn and Virginia Reed Osborn, née Sturges, both of New England stock. The early American Osborns are recorded in several Massachusetts towns, particularly in Salem. The Sturges family had "lived for generations" in Fairfield, Connecticut. One of his mother's ancestors, the Reverend Ebenezer Pemberton, a prominent divine, was a founder of the College of New Jersey (Princeton University). His maternal grandfather, Jonathan Sturges, was a prominent merchant in New York.

His father, although the son of a man of property, had left school at an early age and entered an East Indian trading house in Boston. Sent as their representative to Manila, he was soon in business there for himself. Later he was connected with the Illinois Central Railroad and eventually became its long-term president, engineering it through the panic of 1857 and gradually building up a considerable fortune.

It will be noted that Henry Fairfield Osborn was born in the year of the great panic and four years before the beginning of the Civil War. Although he received his middle name from the town of his mother's ancestors, his parents lived for the most part in New York City, where they were among the "first families."

Henry Fairfield attended the Columbia Grammar School in New York City, where, according to his own recollections, the discipline was rigid.

The summer home of the family was at Garrison, N. Y., near the base of a mountain in the highlands of the Hudson River. There he and his brothers rode and walked along the mountain paths. One of his brothers, who was drowned in the Hudson River near Garrison while still a youth, had made a collection of local birds which was later presented to the American Museum of Natural History in New York. Henry Fairfield, however, unlike most boys who became famous naturalists, does not

appear to have been a "born collector" of insects, rocks, plants or animals. Nor do we hear of his doing other things that implied the patient acquisition of manual skill. His greatness was to be developed in a quite different way.

Later his father built a new and greater mansion on the summit of the same mountain, overlooking the Hudson River, opposite West Point. It would be hard to find anywhere a more delightful view of placid river and rounded mountains than that which is seen from the gray towers of "Castle Rock." Up the winding road to this castle came many American and foreign guests, including men and women who were eminent in science, literature, art and education. But whether at Garrison or in New York City, Henry Fairfield lived in a highly selective social environment which was in many essentials similar to that of the English upper classes; this environment doubtless prepared him in part for his later successful relations with his English friends and colleagues; it also helped to develop the quality of leadership, which was of the utmost importance in his life.

William Henry Osborn gave his sons every advantage and was ready to help them at critical times, yet he took care to train them in habits of industry and self reliance. Thus he encouraged Henry Fairfield, when the latter was a shy and retiring youth of fourteen years, to start "The Boys' Journal", of which thirteen numbers were published. The boy and his fellows prepared the copy, set up and printed the magazine. In his old age Osborn spoke of this experience as a great training in self reliance.

In commenting on the present biographical memoir, Professor Osborn's brother, Mr. William Church Osborn, under date of August 5, 1937, writes as follows:

"His father took a very liberal view of my brother's scientific work and encouraged him in it as far as he could and I think he had him take a year in an office to give him some business experience. He built for my brother the Woodsome Lodge at Castle Rock in which my brother did a large part of his creative work. I think the most important part though was his own example as a persistent, hard-working, hard-driving man of affairs, coupled with an unusual breadth of view on the problems

of the time. I think his example had a great effect on my brother's life."

Henry Fairfield's mother, Virginia Sturges Osborn, was a woman of genuine piety and humanity, who took a prominent part in works of charity and devotion in New York. Concerning the influence of Osborn's mother upon her son, his brother, William Church Osborn, in answer to an inquiry from the Reverend Henry S. Coffin, wrote as follows (December 3, 1935):

"I like to think that the 'long background which explains a man's view' was, in my brother's case, the deeply religious nature of my mother and the training which she gave my brother both by precept and example. She gave a great deal of time to him through his boyhood and said to me once that he was 'the child of many prayers'. I never talked with him on these subjects but my feeling is that with a religious spirit inherited from his mother and his grandfather, Jonathan Sturges, he had the added force of the precept and example of his mother during his formative years."

From the personal influence of both his parents he doubtless derived somewhat of his sincerity and directness, his lack of guile, his urbanity, his loyalty in friendship and his appreciation of loyalty in others. And under the guidance of his parents and teachers, his pastors and friends, he absorbed many of the best features of the religious and social environments of New England, of the "Old New York" families and of the Scotch Presbyterians.

AT PRINCETON

After being prepared for Princeton at the Collegiate Institute in New York, he became a student at that ancient stronghold of democracy and Presbyterianism in the days of the famous Dr. McCosh. A number of the essays of his senior year are preserved. His handwriting at this period was close to its definitive and nearly constant form, which was later described by a French colleague as *"le main magistral de M. Osborn."* If he had ever struggled with timidity and an inferiority complex, if he had ever had occasion to regret impetuous actions, this ponderous handwriting, with its deliberate and very distinguished letters,

its implied earnestness and sincerity, suggests that he had himself well in hand.

One of his essays of this period includes an attempted refutation of the "atheists' argument" that an all-wise and benevolent God would never have permitted evil in the world nor have condemned multitudes of the poor creatures of his own hand to a burning hell. His argument in brief is that, for reasons unknowable to mortals, salvation can be attained only by unceasing effort; this indeed was the theme of his life.

Dr. McCosh knew well not only how to inspire respect for religion in his students but even how to reconcile religion and science. It is said that he was the first Presbyterian divine in America to accept evolution as God's method of creation. And his pupil Henry Fairfield Osborn defended the same thesis. Dr. McCosh was essentially a religious philosopher and teacher, concerned with the relations of mind and matter and the problem of man's origin, fall and redemption. He had a great part in making Osborn also primarily a philosopher and teacher who consistently fought for "idealism" in opposition to "materialism" of the type exemplified by Ernst Haeckel.

As to the sources of his own scientific career, Osborn, in his frank and sincere autobiography,[1] tells us that "throughout this exposition [of his fifty-two years of activity in different fields of science] there is no indication of a predisposition on my part to this life of research; in my school and early life I was never conscious of such a predisposition nor am I able in a review of my ancestry and of my own boyhood to account for my life vocation. My boyhood and youth were similar to those of almost any other boy, undistinguished by a display of the driving force which from the moment of its awakening in the junior year of my Princeton days, has ever impelled me with constantly increasing power. The impulse which led me to dedicate my life to research must truly have come from within. . . . Thus, except for a few early statistics, my biography actually begins with the first call to biology and geology."

This passage reflects, even though indirectly, his life-long leanings toward predetermination, especially by heredity, his

[1] "Fifty-two Years of Research, Observation and Publication: 1877-1929," p. 151.

belief in "creative evolution," in a somewhat mystical sense, and his strong disbelief in "chance" as a factor in personal history.

A more pragmatic account of the young Osborn's "first call" to a life of science has been given in detail by his partner, Professor W. B. Scott. The gist of the matter was that three students in Professor Guyot's geology course, H. F. Osborn, W. B. Scott, and Francis Speir, Jr., determined not to let Yale University have a monopoly in fossil-hunting expeditions in the West. They wisely began by making a preliminary geological reconnaissance of the Highlands of the Hudson River. When Osborn was twenty years old they initiated and carried to a successful conclusion the "Princeton Scientific Expedition of 1877." Under the direction of Professor Kargé, they made collections of fossil fishes and plants in Colorado and of fossil mammals in the Eocene formation near Fort Bridger.

During the ensuing year, as graduate students in the E. M. Museum of Geology and Archaeology, they worked the fossils out of the matrix, searched the reports already published by Leidy, Cope and Marsh, compiled a systematic catalogue of the Eocene Vertebrates of Wyoming, made the drawings and wrote the descriptions of the new material.

In their letter of transmittal of the report [2] to Professor Arnold Guyot, Director of the E. M. Museum of Princeton, they observe:

"Now that the present work is ready for the press, we are very sensible that it must contain errors, which, while they have escaped our notice, will be readily detected by eyes more experienced. These, we trust, will be excused when it is remembered that we are just entering a field which others have explored for years, and opening a work which Princeton, with her many other lines of study, has never hitherto attempted." They also express their "gratitude to Professors Leidy and Cope for their generous aid, both in the way of advice and of material put in our hands for comparison." Thus began the friendly relations with Professors Leidy and Cope, which were to be of great aid to the young palaeontologists in the years to come.

[2] "Palaeontological Report of the Princeton Scientific Expedition of 1877," by Henry F. Osborn, Wm. B. Scott, and Francis Speir, Jr. Contrib. E. M. Mus. Geol. and Archaeol. of Princeton College, No. 1. 1878.

The plates illustrating the report were based on original drawings, mostly by Osborn. They reveal high ability to grasp and record the subtle contours of fossil bones and teeth.

The fossil mammals described represented families upon which Osborn and Scott later spent years of research. There were primates, creodonts or archaic carnivores, perissodactyls, artiodactyls, amblypods, and rodents. Some of them, like *Orohippus,* pointed toward modern families; others, such as *Palaeosyops,* belonged to wholly extinct groups. Thus, Osborn and Scott, together with several of their classmates, made their entry into a world of powerful enchantment, a world which laid such a spell upon them that they were inspired to go back to it again and again.

We find Osborn leading a second Princeton expedition to Wyoming in the following summer (1878) and, in company with Messrs. McMaster, Scott, Speir, McCosh and Annin, exploring the Upper Eocene beds of the Washakie Basin. Here they discovered much valuable material of Professor Cope's *Loxolophodon* and allied genera. These strange beasts had feet like those of elephants but long narrow skulls surmounted by three pairs of blunt horns. They had great sabre-like upper canine teeth and their cheek teeth bore oblique cross-crests adapted for cutting coarse vegetation.

Other important fossils were discovered which were also described later by Osborn [8] and Scott and Osborn. On this expedition the explorers paid more particular attention to the stratigraphic sequence of the Eocene formations of Wyoming and to the problems involved in determining and correlating the corresponding formations in different areas.

Thus we suspect that the mysterious "impulse" and "call" to a life of research which Professor Osborn envisioned in his later years may have been not a sudden "creative evolution" but a gradual summation of his own reasoned responses to favorable environmental stimuli and opportunities.

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[8] E.g., "A Memoir upon *Loxolophodon* and *Uintatherium.*" Contrib. E. M. Mus. Geol. and Archaeol., College of New Jersey, vol. I, no. 1, 1881.

See also Contrib. E. M. Mus. Geol. and Archaeol. Princeton College, Bull. No. 3, 1883.

Not yet, however, was Palaeontology a potent word in the curriculum of the College of New Jersey. Biology, on the other hand, was just beginning to acquire its magic. In short, Dr. McCosh, with his broad philosophy, saw that there was room for both Scott and Osborn in that department. They in turn had realized their own lack of adequate preparation in biology and their need of direct contact with the science that had been so recently rejuvenated by Darwin. What more natural than that they should plan a year of study in Europe? Meanwhile, in the winter of 1878 Osborn [4] took a special course of study in anatomy and histology in the College of Physicians and Surgeons and Bellevue Medical School of New York under Dr. William H. Welch, who later became famous at Johns Hopkins University.

In April, 1879, Osborn went to Cambridge University, England,[5] and there joined Scott in a three months' study of embryology under Francis Balfour. In the Morphological Laboratory at Cambridge, Balfour supervised the studies of the young Americans upon certain developmental stages of the common European newt.[6] This study must have been completed within a few months of its inception, for it was published in October of the same year. Taking the plans and descriptions followed on the one hand by Balfour in his account of the embryology of sharks, and on the other hand by Götte in his description of the development of the toad (*Bombinator igneus*), the authors dealt in a strictly comparative way with the segmentation and formation of the three germ layers, the origin of the notochord, the body cavity, somites of the head, etc. The plates were made from drawings by the authors.

The summer of 1879 Osborn spent in Germany [5] and the winter of 1879-80 in London, studying comparative anatomy at the Royal College of Science under Professor T. H. Huxley. This was in many ways a period of signal importance to his future career. From every one of his great teachers he derived inspiration, perspective, insight and method.

[4] "Fifty-two Years of Research, Observation and Publication," p. 153.
[5] "After Twenty Years": The Record of the Class of 1877, Princeton University, 1877-1897, p. 72. Trenton, N. J. 1898.
[6] Quar. Journ. Micros. Sci., vol. XIX, N. S. London, 1879.

While his Princeton and home connections gave him an introduction into the best social and university circles, he and his partner Scott were likewise welcomed as young explorers of the fossil treasures of western North America. And we can well imagine how deeply the sensitive young Osborn must have been moved when Professor Huxley, conducting Charles Darwin through the laboratory of comparative anatomy in London, stopped and presented the young American to that incomparably great and gentle man. On this and succeeding periods in England he not only formed friendships of great value, with Professor Huxley, with Francis Galton, Edward B. Poulton, Leonard Darwin, and many others, but also acquired an abiding interest in the grand problems of evolution.

Returning to Princeton in the autumn of 1880, Osborn [7] was appointed to a special Biological Fellowship. In 1881 he received the degree of Sc.D. and was made Lecturer in Biology, then Assistant Professor and finally Professor of Comparative Anatomy.

During the twelve years of his service at Princeton his scientific publications, which were few and small in comparison with those of his later years, dealt mostly with topics in mammalian palaeontology, in comparative neurology and in certain limited studies of the "mind's chamber of imagery," due to connections with Dr. McCosh.

In the earlier years he frequently read papers at the Science Club at Princeton, some of which are preserved [8] in manuscript form. One of them is a brief unpublished account of a journey that he made shortly after his return from England to the coastal lands of Texas and Louisiana to collect materials for his and Scott's studies on the development of the Amphibia and Reptilia and for his own studies on the evolution of the brain. The country over which he travelled on horseback and by coach abounded in interest to the young naturalist: here ancient amphibians lived and bred, alligators laid their eggs, covered them in mounds and left diverse footprints on the muddy banks; here ganoid fishes of vast antiquity left their scales and bones in the

[7] "After Twenty Years": The Record of the Class of 1877.
[8] In the Osborn Library of the American Museum of Natural History, New York.

mud; here, in short, the story of past ages was being reenacted before his eyes.

Nevertheless he seems to have regarded this journey as unsuccessful. "Balfour's inspiration in embryology," he tells us,[9] "led to unsuccessful journeys and attempts in the southern United States to secure the embryonic history of two very characteristic American types, namely, the alligator and the giant long-tailed amphibians *Amphiuma* and *Menopoma*—unsuccessful because I had not the aptitude of the field naturalist in collecting these eggs and embryos nor a talent in embryological technique. It is well to realize one's inaptitudes, so that one may the better advance in the direction in which his talent does lie. *If you cannot find your research way in one direction turn to another in which you may be successful.*"

Whether or not he was justified in being discouraged and in believing that he "had not the aptitude of the field naturalist in collecting these eggs and embryos nor a talent in embryological technique," this state of mind may have disposed him to withdraw from comparative embryology and devote more time to fossil mammals; the latter by 1888 had practically crowded out his further work in the former field.

Here again we see in his history no signs of "predestination," hereditary or otherwise, but chiefly a wise opportunism and adaptability, a realization of his limitations in certain directions and a growing copiousness of production along the lines which suited him best.

By the time Osborn returned from England, after his contacts with Balfour and Huxley, he had a perfectly clear field of major interest, which was the study of evolution from as many angles as possible. Accordingly he pushed forward his studies on the foetal membranes of the opossum and other marsupials (1883, 1887) and upon the structure and evolution of the brain in amphibians (1883-1886). He was never content merely to describe facts but sought always for results of general interest. In this connection, in his autobiographic sketch (p. 63) he writes as follows:

"In every field of nature direct observation with accumulation

[9] "Fifty-two Years of Research, Observation and Publication," p. 65.

of facts is a fascinating pursuit. Many most useful and even eminent naturalists spend their entire lives in this way; they are the 'drawers of water and hewers of wood' for others. It is not that they lack powers of generalization but that they apparently take no interest in generalization for the drawing of inductions. Sometimes important discoveries are made in this methodical way but more often discovery of new principles emerges from generalization. In any event, the *discovery of new principles is the chief end of research.* As Darwin observed, the true naturalist is not content with merely assembling facts; his chief desire is to seek interpretations and explanations —in other words, to discover new principles.

"I have myself always found the mere assemblage of facts an extremely painful and self-denying process and I have always been animated by the hope that such dry work would finally be rewarded by an interpretation or the discovery of a new principle."

In the same revealing source book (p. 65) he sums up his work on the internal structure of the amphibian brain in the following passage:

". . . From personal experience also I discovered a very important principle in the internal structure of the amphibian brain (1888), namely, *the subdivision of the cranial nerves into many separate physiological components,* which in these very small brains could be traced with great distinctness, so that the older classification of the cranial nerves could be amplified by the newer classification based upon the many separate components.

"This near vista awaited the masterly patience and technique of one of my students, Oliver S. Strong, now Professor of Neurology in Columbia University, in a classic memoir on the brain of the frog, in which all the components of the cranial nerves were skillfully traced. A similar classic on the brain of the fish was also prepared by C. Judson Herrick, of the University of Chicago, under Strong's direction. My ten-year experience in comparative anatomy and neurology thus gave me the privilege of initiating a new school of neurological research in America, in which I myself saw the Promised Land in the

distance, although my technical disability and preoccupation with other interests precluded my attaining it."

His search for the origin of the mammalian corpus callosum led him to conclude (1886) that this structure was already foreshadowed in the brains of amphibians, but later neurologists [10] were able to prove that the corpus callosum arose only in the mammals above the grade of the monotremes.

In his work on the embryonic membranes of the opossum, "Osborn's only serious error seems to have been his identification of actual 'villi' on the outer surface of the chorion (subzonal membrane) in the region where the yolk-sac comes into apposition to its inner surface. These 'villi' have not been observed by any one else, and it seems almost certain that they were mere blister-like artifacts. In Osborn's 1883 paper he does not speak of these villi as already vascular but conjectures that they later become so. (He was misunderstood on the point by Haddon in his *Introduction to Embryology*, 1887.) The material on which Osborn's 1883 paper was based was very limited; only one pregnant opossum, and one horn of the uterus with the contained embryos was accidentally lost. His observations on the vascularity of the yolk-sac and his suggestion of its importance as the chief organ of nutrition during intrauterine life of the embryos are highly important, and he speaks of the structures as 'yolk-sac placentae'" (J. H. McGregor).

"Osborn thought, apparently erroneously, that the *cavity* of the yolk-sac in *Didelphys*, between the inner vascular surface and the outer non-vascular surface, became obliterated. This is certainly not the case in other marsupials" (J. H. McGregor).

Continuing his fruitful partnership with Scott, he published several papers (1882-1887) on Eocene ungulates, culminating in the account (1887) of the fossil mammals of the White River formation. In this paper the authors briefly described several new species of certain giant extinct ungulates that were allied with Dr. Leidy's *Titanotherium*, Cope's *Symborodon* and Marsh's *Brontotherium*. At this time doubtless Osborn began to be interested in the confusing taxonomic problem of this strange family (afterward referred to as the "titanotheres"),

[10] Elliot Smith and others.

which was to occupy so many years of research at a later period.

In 1886 Osborn made an extended visit in England in order to study the remains of Mesozoic mammals, chiefly in the British Museum. These remains consisted almost wholly of numerous broken jaws and teeth. The smallest of these animals were no bigger than mice, the largest scarcely as big as kittens. They had been monographed many years before by Sir Richard Owen and catalogued and revised by Lydekker. Moreover, Professor Marsh had recently published a paper describing corresponding forms from the dinosaur-bearing beds of Jurassic age in Wyoming. Nevertheless the young American, somewhat rashly perhaps, made his own independent studies on the originals and the results,[11] imperfect as they undoubtedly were, fully justified his efforts. For in the first place it was well that these oldest known relics of the mammalian class should be so carefully restudied by a young mind and from the evolutionary viewpoint; and although, as Osborn recognized, the fossil record was still too imperfect to give final answers, yet the fragments did establish the facts that at a period as far back as the lower Jurassic the multituberculate mammals were already an ancient and highly specialized side branch, while the remaining families were diversely specialized away from a still more ancient primitive insectivorous type; also that some of them seemed to be more nearly related to the stem of the marsupials, others to the earliest insectivores of the placental series.

Secondly, Osborn's work on the Mesozoic mammals appeared to him to have brought considerable support to Professor Cope's view of the way in which the "tritubercular" type of molar tooth seen in many of the Lower Eocene mammals had in its turn arisen from the single-tipped molars of the Upper Triassic *Dromatherium*.

After studying the teeth of these oldest known true mammals, the contemporaries of the older Jurassic dinosaurs, Osborn went on to develop and apply Cope's theory in a series of papers dealing with the "Evolution of Mammalian Molar Teeth to and from the Triangular Type," which were eventually (1907) brought

[11] "The Structure and Classification of the Mesozoic Mammalia." Journ. Acad. Nat. Sci. Philadelphia, vol. IX, no. 2. 1888.

together in the well known book of that title. Thus he became the founder with Cope of what is commonly called the "Cope-Osborn theory of Trituberculy," which has been of incalculable value to vertebrate palaeontologists, since it placed in their hands what Osborn called a key to the complexities of the highly diversified patterns of the molar teeth of mammals. Osborn's fruitful labors in this field must here be dismissed with the foregoing lines but they have been elsewhere summarized by the present writer.[12]

For many years prior to Osborn's work on Mesozoic mammals Cope and Marsh had been engaged in their strenuous rivalry and struggle for prior discovery and publication of the fossil vertebrates of the West. Osborn and Scott even in their first "Palaeontological Report of the Princeton Scientific Expedition" were almost inevitably ranged on the side of Cope, from whom they received much encouragement and practical assistance. For example, after describing the skull and dentition of their supposedly new genus and species, "*Leurocephalus cultridens*," they write (p. 48): "This may eventually prove to be a species of *Telmatherium* (Marsh); but the description given by him is so brief and uncharacteristic that it might apply to any of the allied genera. Indeed, Dr. Leidy has regarded it as a synonym of *Palaeosyops*." Again, in his first independent memoir, that on *Loxolophodon* and *Uintatherium* (1881), Osborn implied that Marsh followed Leidy in the exploration of the Bridger beds, a statement which was flatly contradicted by Marsh in his memoir on the Dinocerata (1886, p. 236).

Whatever may have been the merits of the original controversy, it seems to have led, at least indirectly, to a serious clash between Osborn and Marsh over the Mesozoic mammals. Osborn in 1891 published a critique of Professor Marsh's "Discovery of the Cretaceous Mammalia," in which he tried to show that Marsh had applied a great many generic and specific names to the isolated parts of the highly differentiated dentition of a single type of multituberculate. Marsh replied ("Note on

[12] "A Half Century of Trituberculy: The Cope-Osborn Theory of Dental Evolution." Proc. Amer. Philos. Soc., Vol. LXXIII, No. 4, April, 1934, pp. 169-317 [Osborn's theory and diagrams, 181-186].

Mesozoic Mammalia") with a vitriolic review of Osborn's monograph on the Mesozoic Mammalia, to which Osborn in turn (1891) very soon came back with a vigorous rejoinder. A careful reading of the documents impresses the present writer that Osborn more than held his own in this affair and that he revealed himself as by no means unwilling to defend his right.

After Osborn's vitalizing contact with the English leaders in evolution it is not surprising that he retained an abiding interest in all the larger questions of evolution, such as the relations between natural selection and the Lamarckian factor of use and disuse. It was not until 1889, however, that he began a long series of communications in this field with his paper on "The Palaeontological Evidence for the Transmission of Acquired Characters." The series was continued at intervals to the close of his life. It is so intertwined with more limited subjects that one finds difficulty in separating the titles of his papers that deal with the factors of evolution from those treating of the historical evidences of the process. Thus he himself (1930, pp. 101-106) lists no less than eighty-five titles under the head of "Biology and New Principles of Evolution," after excluding all those that dealt with the evolution of man and with "Evolution and Religion in Education." However, many of the entries on his list are secondary sources, such as reviews or accounts of his addresses, interviews and popular articles in the daily press, and the like. Nevertheless, after all necessary deductions have been made, his publications on the general principles of evolution reach a formidable total. His main conclusions on the ways of evolution are summarized below (p. 82).

AT COLUMBIA UNIVERSITY

From 1873, when the young Osborn entered Princeton, to 1891, he was in residence there except for brief periods abroad. He was married [13] on September 29, 1881, to Miss Lucretia Thatcher Perry, daughter of General Alexander J. Perry, U. S. A., and was the father of five children. He had a delightful home in Princeton and every prospect of remaining

[13] "After Twenty Years . . . ," p. 74.

peacefully there as Professor of Comparative Anatomy and free to follow his own bent in palaeontology for the rest of his life. Then, when he was only thirty-four years of age, came the call to Columbia (1891) and the simultaneous call to the American Museum of Natural History. To Columbia,[14] at that time in the early stages of its transformation from a small college to a great university, he was called by President Seth Low in order to plan, establish and head the new department of biology. To the American Museum [15] he was called by President Morris K. Jesup to found and guide a department of mammalian palaeontology. Perhaps no one now living knows the exact steps by which these two calls were so completely synchronized and integrated but the twofold relationship proved to be most fortunate and far reaching.

At Columbia the legacy of Charles M. Da Costa made it possible to develop the new department of biology upon a liberal scale. The department was organized under Professor Osborn's direction in 1891,[16] but it was not opened until 1892. Professor Osborn was made Da Costa professor of zoology; Professor Edmund B. Wilson was called from Bryn Mawr for invertebrate zoology; Dr. Bashford Dean of Columbia University, who had been associated with Professor John Strong Newberry in the study of Palaeozoic fishes, was made instructor in vertebrate zoology. For the first six years the department was carried on in a laboratory at the College of Physicians and Surgeons.

Early in 1892 Professor Osborn delivered a series of three lectures on "Present Problems in Evolution and Heredity." These were addressed to the alumni of the College of Physicians and Surgeons. They constitute an admirable summary and analysis of current literature bearing on the following, among other topics: the meaning of "anomalies" and "reversions" in human anatomy, the relations of development, balance and

[14] "A History of Columbia University," 1754-1904. . . . New York, 1904, pp. 155-156.

[15] The American Museum of Natural History . . . : Annual Report of the Trustees. . . . Twenty-third Annual Report of the President, 1891. New York, 1892, p. 10.

[16] "Zoology at Columbia." Columbia University Bulletin, December, 1897.

degeneration, chiefly in human bones and muscles; history of the heredity theory, contrasting claims of Lamarckian and Weismannian schools; factors of evolution, difficulties in the natural-selection and use-and-disuse theories; nature of the relations between the body cells and the germ cells; phenomena of cell division, including distribution of the chromatin substance to the tissues; nature of fertilization, and the like These topics were destined to be dealt with later in great detail by others in Professor Osborn's new department and it was characteristic of him that he undertook the great labor of preparing such a series and that he was measurably successful in correlating so many specialities in a broad, comprehensive, and at the same time well documented presentation.

The breadth of scope of Professor Osborn's lectures at Columbia, together with his excellent blackboard drawings, aroused the enthusiastic appreciation of both undergraduate and graduate students. He was soon elected the first Dean of the Faculty of Pure Science in Columbia University, which position he held for three years.[17] In the meantime he had supervised several scientific expeditions by members and graduate students of the department and had instituted the Columbia University Biological Series of books, outlining its scope and editing it for several years. Volume I of the series, his own "From the Greeks to Darwin: An Outline of the Development of the Evolutionary Idea," was based on a series of lectures which he had delivered on this subject. In it he traced the history of the idea of evolution from its more or less obscure adumbrations among the Greek philosophers and the fathers of the Church to the more immediate predecessors and contemporaries of Darwin. With regard to the place of Aristotle in the history of the discovery of evolution, Osborn concluded (p. 57) that "Aristotle had substantially the modern conception of the evolution of life from a primordial soft mass of living matter to the most perfect forms, and that even in these he believed evolution was incomplete for they were progressing to higher forms." This interpretation of Aristotle's views has, however, been

[17] "Fifty-two Years of Research . . . ," p. 153.

shown to be incorrect by Harry Beal Torrey and Frances Felin,[18] for although Aristotle recognized the existence of intermediates, for example, the ascidians, as supposedly partaking of the nature of both plants and animals, yet he was a firm believer in the Platonic doctrine of eternal "forms," including what are now called organic species. Osborn, in short, put together Aristotle's recognition of individual development and of gradational characters between otherwise distinct forms and then imputed to Aristotle a belief in phylogenetic progression.

While Columbia was preparing to remove to its new site at Morningside Heights, Professor Osborn and his colleagues in the department of biology spent a great deal of time and effort in planning the new laboratories, library and offices of the department, which were to be located in the Natural Science building presented by William C. Schermerhorn. All this was successfully done; Professor Osborn delivered an address, "The Corner Stones of Learning," on the occasion of the dedication of the new site at Morningside Heights on May 2, 1896, and supervised the installation of the department in its new home in 1897.[19]

Under his leadership a number of important contributions were made to various broad problems of evolution in which he was particularly interested. Dr. Oliver S. Strong published a detailed analysis of the brain and cranial nerves of the frog. Dr. Arthur Willey prepared a valuable work on "*Amphioxus* and the Ancestry of the Vertebrates." Dr. Bashford Dean produced his remarkable textbook on "Fishes, Living and Fossil" and his colleague Dr. Edmund B. Wilson published his great work on the Cell.

Osborn continued to give regular lectures in his Columbia courses on the Evolution of the Vertebrates and on the Evolution of the Mammals until about 1907, but gradually, owing to increasing pressure of other work, he handed over his courses to the present writer, who was for many years his assistant at both Columbia University and the American Museum. Mean-

[18] "Was Aristotle an Evolutionist?" Harry Beal Torrey and Frances Felin in Quarterly Review of Biology, Vol. 12, No. 1, March, 1937, pp. 1-18.

[19] "Zoology at Columbia," Columbia Bull., December, 1897.

time his graduate courses had been transferred to the department of vertebrate palaeontology at the Museum in furtherance of a plan adopted by both institutions for close coöperation in this matter. Osborn was made research professor of vertebrate palaeontology at Columbia but generously arranged for the reversion of his salary to the department of zoology there.

AT THE AMERICAN MUSEUM OF NATURAL HISTORY

A. As Curator

Returning to the other main stream of Professor Osborn's activities, we find him losing no time in organizing his department of mammalian palaeontology at the Museum, which opened in May, 1891, and in sending an expedition to the Lower Eocene beds of Wyoming. His first assistant was Dr. J. L. Wortman, one of Cope's former collectors, a man well trained in human anatomy, in field geology and in the technique of taking out fossils from their stony beds. Osborn's second assistant was Charles Earle, one of his graduate students at Princeton, who was then busy on a memoir on the genus *Palaeosyops* and its allies. Another assistant was Mr. O. A. Peterson, who eventually became curator of palaeontology in the Carnegie Museum of Pittsburgh.

The first expedition of the department was highly successful in the discovery of fossil remains; the report on these by Osborn and Wortman in 1892 deals with: the "homologies and nomenclature of the mammalian molar cusps (H. F. O.), the classification of the Perissodactyla (H. F. O.), the ancestry of the Felidae (J. L. W.), the taxonomy and morphology of the Primates, Creodonts and Ungulates (H. F. O.), geological and geographical sketch of the Big Horn Basin (J. L. W.), and Narrative of the Expedition of 1891 (J. L. W.)," each under the individual author's initials. Here we see Osborn, who had collaborated so harmoniously with Scott but had been forced by his removal from Princeton to dissolve that partnership, continuing this general policy of partnership, first with Wortman, then with Earle, later with Matthew and with many others, to

whom he never failed to give generous acknowledgment as well as access to priceless material.

To return to the first report on the collection of Lower Eocene mammals, the joy of discovery shines on almost every page and the cautious following of the elders which was evident in the early palaeontological papers has now given place to confidence based on previous triumphs. One is tempted to comment on many of the ideas in this and succeeding palaeontological reports in the American Museum Bulletins but there is space for only the following item. After describing the construction of the fore foot in the oldest known species of *Palaeosyops,* Osborn writes: "In short, this foot is distinctly mesaxonic and functionally tridactyl, whereas the later forms from the Bridger are, so far as known, paraxonic and functionally tetradactyl; in other words, we find an early species with a more progressive and modified form of foot than the later species, a state of affairs which is decidedly inconvenient for the evolutionist." It remained for Dr. W. D. Matthew, one of Osborn's graduate students at Columbia, later Assistant Curator and eventually Curator in the Museum, to bring forward the palaeontologic evidence that as regards the evolution of the fore foot in perissodactyls, Cope, followed by Osborn, was looking at evolution through reversed mental glasses; that is, they had mistaken what was really the older and more primitive condition for an advanced specialization. Osborn himself was well aware of the human proneness to error and never took it badly when his assistants, whom he had chosen because of their originality, differed from him.

In 1895 the title of the department of mammalian palaeontology was changed to department of vertebrate palaeontology, as better defining the more inclusive character of its collections.* This year was notable for the purchase of Professor Cope's great collection of fossil mammals of North America, including nearly 10,000 specimens representing 483 species from the chief horizons of North America. For the gift of this collection the Museum was indebted not only to President Jesup and other

* Annual Report of the President for 1895, American Museum of Natural History, New York, 1896, p. 16.

trustees but to Professor Osborn's father, William Henry Osborn, as well as to Professor Osborn himself. Meanwhile, year after year, Museum expeditions continued to discover new fossil mammals in the Rocky Mountains and Great Plains.

The subsequent history of the department of vertebrate palaeontology under Osborn's leadership would require a volume in itself and even if we limit our attention to the chief works in which Osborn himself took a particularly active part, the list would be too long for more than passing comment. However, one may mention the following:

(1) The discovery of numerous dinosaur remains in the course of many Museum expeditions in the Rocky Mountains and in Alberta, chiefly under Barnum Brown; described in the memoirs and bulletins, by Osborn and his assistants, on *Diplodocus, Brontosaurus, Morosaurus, Tyrannosaurus, Ornitholestes, Struthiomimus,* and others.

(2) The studies on the ordinal classification of the reptiles by Osborn and McGregor, which resulted in the proposed subdivision of the entire class into two subclasses, Diapsida and Synapsida. This classification became the stimulus for many others by subsequent authors.

(3) Osborn's studies on the origin of the mammals from the mammal-like reptiles and on the origin of birds from the Permian ancestors of the dinosaurs, although not based on specimens originally described by him, were marked by insight and good sense and have not yet been superseded except in details.

(4) His studies on the classification and phylogeny of the fossil rhinoceroses of Europe and North America, leading to: (a) his recognition of the differences in the length-breadth proportion of the skulls (in "dolichocephalic, mesaticephalic and brachycephalic" types), and of the feet (in "dolicho-mesati- and brachypodal" types); (b) his use of these characters as diagnostic of numerous subfamilies; (c) his conclusion that, contrary to his earlier beliefs, the rhinoceroses were exceedingly "polyphyletic" during the greater part of the Tertiary period.

(5) His studies on the classification and evolution of the titanotheres, culminating in the great two-volume monograph on this family, which was completed in 1920 and published by the

United States Geological Survey in 1929. Here the principles used in the rhinoceros studies were further developed and the taxonomic results were made the basis for many conclusions regarding the ways in which evolution has taken place. The history of the family was followed in great detail from the later part of the lower Eocene through the middle and upper Eocene to the top of the lower Oligocene. The fossils show that during this time the race increased in size from animals that were about as large as racehounds to huge beasts rivalling the modern elephant in bulk, though not in height. Although divided into many parallel and divergent lines, all became extinct, at least in North America, by the close of lower Oligocene times.

In seeking answers to the question, "What may have caused the sudden extinction of this race?" Osborn searched the vast literature of extinction in animals and compiled over one hundred causes and conditions that may have contributed to the extinction of different kinds of herbivorous mammals. The extinction of the titanotheres he attributed in part to the fact that in them the evolution of the cheek teeth was far more retarded than in other perissodactyls and he supposed that they were crowded out by more efficient competitors.

(6) Osborn's continuous studies on the correlation of fossil mammal-bearing horizons of Europe and North America, which were begun in his earliest memoir (on *Loxolophodon,* 1881), together with his many publications on the fossil mammals of the Tertiary, supplied the basis for his textbook, "The Age of Mammals in Europe, Asia and North America" (1910), which, although compiled with the aid of several assistants, constitutes one of his greatest and most useful works.

(7) For many years Osborn supervised the work of assistants in compiling a vast deposit of excerpts from the scientific literature of the species and breeds of horses, asses and zebras. It was his intention to work this up into a monograph on the Equidae, living and fossil, but other interests, particularly the Roosevelt Memorial and the Proboscidea Monograph, prevented him from realizing this ambition; consequently a great part of the material he amassed still lies untouched. However, his efforts in this direction led to three important results:

(a) Mr. William C. Whitney provided funds for exploration and research by the aid of which the Museum's collection of fossil Equidae received many important accessions; Mr. J. W. Gidley, under Osborn's direction, secured among other valuable material a beautiful skeleton of *Neohipparion whitneyi.*

(b) Mr. Samuel Harmsted Chubb was engaged in 1901, and even at this writing (1937) is still engaged in preparing a superb series of mounted skeletons representing the principal species and breeds of horses, asses and zebras and illustrating characteristic postures and movements of the animals in life; at the same time a great study collection of recent Equidae skeletons and dentitions has been built up.

(c) At Professor Osborn's request, Dr. W. D. Matthew prepared the first draft for a revision of the fossil Equidae of the Oligocene, Miocene and Pliocene of North America, accompanied by an extensive series of excellent illustrations. This was intended to have been a joint work by Osborn and Matthew but the latter adhered firmly to the so-called "horizontal" or zonal system of defining and classifying genera, whereas Osborn was an enthusiastic exponent of his own "phylogenetic" or vertical method of putting remote ancestor and descendants in the same "genus." This difference finally resulted in a deadlock, in which Matthew withdrew from the work and refused to collaborate with Osborn. At the same time, however, he handed over all his manuscript and had no objection to Osborn's using it as he saw fit, but he refused to allow his name to be used as co-author of the published work. In this awkward situation Osborn made the following frank and generous acknowledgment in the preface to the memoir.

"The author is chiefly indebted to the original researches by Matthew mentioned above (Matthew, 1913) on the Miocene and Pliocene species of Equidae in the collections of the American Museum of Natural History. At first a joint report was contemplated; but for certain reasons it has appeared wiser that the present revision should appear under a single name. The author issues it, therefore, with the fullest acknowledgment of his indebtedness to his colleague, from whose descriptions, observations, and definitions many quotations are taken entire

without amendment. On certain points of difference of interpretation and opinion which have arisen the author holds himself wholly responsible."

In spite of these regrettable but sincere differences the memoir contains superb material for the study of evolution.

(8) Osborn's studies for "The Age of Mammals" naturally led him to plan a corresponding volume on the Age of Man. In 1912 a rapid motor tour through those parts of Europe that contain the principal sites of the Palaeolithic cultures gave him a general introduction to the subject and three years later appeared his "Men of the Old Stone Age, Their Environment, Life and Art." He was assisted in this work by Miss Christina D. Matthew. The text was illustrated from photographs of the carefully studied reconstructions by Professor J. H. McGregor of the skulls and external features of the head of *Pithecanthropus* and of the Neanderthal and Cro-Magnon men. Further studies, on the correlation of the divisions of the Pleistocene of Europe and North America, were made with the collaboration of Dr. Chester A. Reeds.

"Man Rises to Parnassus" is chiefly based on the studies of Montelius and others on the Neolithic, bronze and iron cultures of Northern Europe, which Osborn also visited.

(9) Osborn's growing conviction of the great antiquity of many "phyla" of mammals gradually led him to the view that the human race had an equally long independent ancestry stretching back perhaps into Eocene times before merging in a common stock with the remote ancestors of the anthropoid apes. Partly for this reason he refused to assent to the theory that man's ancestor had ever been an ape. Vigorously attacking the "ape-man theory" as a "myth," he wrote enthusiastically that humanity should be grateful to the anthropologist for having freed man from the bar sinister of ape ancestry. Against these and similar utterances the present writer felt compelled to enter a vigorous demurrer and on several occasions Osborn and the writer presented these opposing views from the same platform. It was greatly to Osborn's credit that he refrained from using his power to silence his former assistant and that he always treated the latter not only with perfect fairness but with unfail-

ing friendship, so that to the day of his death there was never a cloud between them.

(10) Osborn's last and in many respects his greatest work was the monograph on the Proboscidea, projected in 1908 but not fairly begun as a monograph until 1920. The first volume, including 802 pages and 680 figures and 12 plates, was published on August 15, 1936, nine months after his death; the second volume, which will appear in 1939, will be of about the same size. The main results are that the order Proboscidea is divided into five superfamilies, eight families, twenty-one subfamilies, forty-four genera and about three hundred and fifty species, including subspecies and varieties, mostly on differences in the molar teeth, incisor tusks, jaws and form of skull. This was perhaps the climax of "polyphyletism."

(11) By no means the least of Osborn's gifts to the Museum was the Osborn Library of Vertebrate Palaeontology, including at the time of his death about 8,000 bound volumes, several thousands of "separata," together with a subject index of possibly 70,000 entries.

B. As President

The annual reports of the President of the Board of Trustees of the American Museum of Natural History from 1908 until 1933 contain a great deal of material relative both to the growth of the Museum and to the ideals which guided that growth during the presidency of Henry Fairfield Osborn. One reads there how, one after another, the new wings that had been planned during Mr. Morris K. Jesup's presidency (namely, all those on West 77th Street and the southwest wing on Columbus Avenue) were completed, and how the exhibition halls therein were opened one by one. And one also reads of the buildings planned under Professor Osborn's presidency, including the buildings along Central Park West, together with the African Wing, the Education Building, the Preparation and Power Building, the Roosevelt Memorial, built by the State of New York; one after another these were completed and connected with the general system. And in a special publication on the plan and scope of the Museum are set forth Osborn's plans for the future development and completion of the entire Museum. But in these publi-

cations the reader receives hardly a hint of the *vis-a-tergo* that was constantly being exerted by President Osborn upon the Museum Trustees and the city and state officials by whom the necessary enactments and appropriations were in every case to be approved and put into effect.

Nor does the reader easily derive from these somewhat formal reports such a vital portrait of Henry Fairfield Osborn, President of the Museum, as was presented the day after his death by Dr. George H. Sherwood, Director of the Museum during the greater part of Professor Osborn's presidency.

"Professor Osborn has left behind him a splendid record of service to the American Museum, covering a period of more than forty years. He was our president for a full quarter of a century, an epoch in the Museum's history which witnessed unprecedented development in all its branches—physical, financial, scientific, and educational. Under his leadership and direction the building space was more than doubled; the city's annual appropriation for maintenance increased from approximately \$160,000 to the half million mark; the endowment, the backbone of the Museum's life, multiplied from \$2,000,000 to more than \$14,000,000; the Museum membership increased over 400 per cent; the scientific staff was more than trebled; the earth was dotted with our expeditions; collections of priceless value were brought within our walls; volumes of publications, scientific and popular, were issued and given world-wide distribution; the artistic standard and effectiveness of Museum exhibitions was vastly improved; while the popular presentation of natural history and kindred subjects was made so appealing that many technical scientific terms became household words.

"Throughout President Osborn's administration it was my privilege and good fortune to stand at his elbow. In this long association, therefore, I had exceptional opportunity to observe, feel, and *know* his character and personality. Words of mine would be wholly inadequate to summarize his achievements or evaluate his contributions to human knowledge. Therefore I wish to speak merely of a few of his characteristics which were outstanding and have left on us a vivid impression of a great man.

"Notable was his *breadth of vision.* While his special science was palaeontology, it was the Museum as a whole that was of paramount importance, and every department had his genuine interest. Always was he keen for developments which would make our institution a greater power in science and education. For example, I well remember his enthusiasm when he first learned of the wonderful instrument called the Zeiss Planetarium. It was only a few weeks later that he dispatched Doctor Fisher to Germany to bring back first-hand information of it.

"Next I have in mind his *tenacity of purpose.* Here was a man born to wealth who might have led a life of luxury and ease. Instead, he set out with the determination to make his mark in the field of science—and he did. This same tenacity of purpose he applied to the administration of the Museum. Obstacles did not deter him. They were there to be overcome and he forged ahead. When he was sure he was right, no one, not even his most intimate friends, could swerve him from what he felt was his path of duty.

"Third I place his *fertility of mind*—and what fertility it was! He could think of enough things that ought and must be done to keep a regiment of us busy. Oftentimes I wished his mind weren't quite so fertile and that we might have time to catch up with his ideas or complete some of the problems on hand. Still it was this fertility of mind that kept us on our toes and urged us on to achievement.

"Another outstanding characteristic was his *sympathy with youth.* He had an affectionate regard for all children and nothing gave him so much pleasure as to see the classes of school children streaming into the Museum. . . .

"Finally I mention the Professor's *eternal optimism.* He had a philosophy of life which carried him over discouragements that would have floored a less virile nature. Again and again I have seen him up against difficulties that were simply overwhelming, and invariably he would come up with a smile on his face. And so I feel that if he were here today he would not want us to consider this a time for sorrow or sadness at the loss of a dear friend, but rather would wish us to reflect on the

achievements of his administration and do our best to carry on his ideals."

Osborn's presidency was important to the Museum also because he constantly insisted on very high standards of beauty in the mounted groups, in mural paintings and other decorative features of the exhibition halls.

HIS LEADERSHIP IN OTHER SCIENTIFIC AND EDUCATIONAL ORGANIZATIONS

Professor Osborn's brilliant successes at Columbia University and the American Museum encouraged other scientific and educational organizations to look to him for leadership. The American Society of Naturalists started the ball rolling in 1891 by electing him as its President.[20] The next year the American Association for the Advancement of Science made him one of its Vice Presidents. The New York Academy of Sciences, the New York Zoological Society, the Marine Biological Association of American Colleges, the American Morphological Society and later organizations, elected him at different times to their presidencies. And a greater presidency overtook him in 1908 when he succeeded Mr. Morris K. Jesup as the head of the American Museum.

Soon after coming to New York he began to take an active part in the meetings of the New York Academy of Sciences, presenting there many of his palaeontological results and serving in 1894-96 as Vice President and in 1898-1900 as President of the Academy. He was especially influential in bringing about the permanent affiliation of the Academy with the Museum and the eventual transfer of the Academy itself to the Museum buildings. His two annual addresses as President of the Academy in 1899 and 1900 dealt extensively with "Correlation between Tertiary Mammal Horizons of Europe and America." In the first address he analyzed the evidences of parallelism between the successive horizons of Tertiary mammals on opposite sides of the Atlantic. In the second address he dealt espe-

[20] "After Twenty Years . . . ," p. 73.

cially with the faunal succession and geographical distribution of the orders and families of mammals on the different continents. This led to his "theory of successive invasions of an African fauna into Europe"; this was that the Ethiopian region [in the broad sense] or South Africa was a great center of independent evolution for the "Proboscidea, Hyracoidea, certain Edentata, the antelopes, the giraffes, the hippopotami, the most specialized ruminants, and among the rodents, the anomalures, dormice and jerboas, among monkeys, the baboons. . . ." These, he suggested, "may all have enjoyed their original adaptive radiation in Africa. . . ."

"On the other hand," he concludes, "certain families had an exclusively Eurasiatic history, so far as we know." These are, among others, "the anthropoid apes. . . ." Again in another passage: "Northern Asia is unknown palaeontologically until the Pleistocene—here is a region for explorers. However, we may consider it as part of a broad Eurasiatic land area—extending from the Rocky Mountain region to Great Britain. The faunal relations are astonishingly close, between the new and old worlds at this time. Every year's discovery increases the resemblance and diminishes the differences between Europe and the Rocky Mountain region."

The next year (1901) Charles W. Andrews of the British Museum began to announce the palaeontological discoveries that were being made in the Fayûm district in Egypt, which proved that at least two early stages in the evolution of the Proboscidea had indeed lived in Africa. The verification of the "prophecy" that northern Asia would be proved to have been part of a broad Eurasiatic land mass, containing important evidence of the faunal interchange between Asia and North America, had to wait until 1922-30, when Roy C. Andrews, another of Professor Osborn's assistants, opened up the great "Lost World" of the Gobi desert.

Professor Osborn was one of the charter members of the New York Zoological Society (1895). In 1896 he presented a joint report, with C. Grant La Farge and Andrew H. Green, embodying a "Preliminary Plan for the Prosecution of the

Work of the Zoological Society." He was chairman of the executive committee from 1896-1903, 1907-1909, President from 1909-1924 and Honorary President from 1927 until his death.

He had a great deal to do with the selection of the site of the Society's Park and with the planning and general style of its buildings and grounds. He was particularly interested in the development of the collection of living proboscideans, perissodactyls and other ungulates. With Mrs. Osborn's unfailing assistance, he was successful in enhancing the social prestige of membership in the Society and attracted many of the leading families of New York to its support.

He gave powerful aid to the Director, Dr. W. T. Hornaday, in the latter's sustained agitation for the protection of native wild life. As Honorary President of the American Bison Society, he assisted Dr. Hornaday in rescuing that species from the extinction with which it was threatened. Ably seconding his friends Madison Grant and John C. Merriam, he was one of the founders of the "Save the Redwoods League," which was finally successful in having certain sequoia-bearing areas set aside as permanent sanctuaries for those noble monuments of long past ages.

Always impartially supporting the researches and expeditions of all members of the staff, he was nevertheless particularly interested in the numerous expeditions and researches of Dr. William Beebe, in the publication of the latter's great monograph on the Pheasants, in the establishment of the Department of Tropical Research, and in the *Arcturus* expedition to the Sargasso Sea and the Galapagos Islands.

To Professor Osborn the animals of the Zoological Society's Park were by no means to be regarded as captives, brought together merely for the amusement of the public. They were, on the contrary, the ambassadors and representatives of the noble races that ruled the world during the Age of Mammals.

Osborn was also active in the American Philosophical Society and the National Academy of Sciences, attending many of the regular meetings and serving on various committees.

SUMMARY OF OSBORN'S VIEWS ON EVOLUTION

The present writer has elsewhere [21] summarized Osborn's chief ideas on the theory of evolution as follows:

(1) The law of *continental and local adaptive radiation;*

(2) The law of *homoplasy* or parallel but independent evolution in related lines of descent;

(3) The law of *tetraplasy,* whereby evolution results not from the operation of single causes, but as the resultant of forces from four principal directions (external environment, internal environment, heredity, selection).

(4) The law of *alloiometry,* or adaptive modification of dimensions of the skull, feet or other parts, arising independently in different lines of descent.

(5) The law of *rectigradation,* or *aristogenesis;* i. e., the gradual appearance during long ages of new structural units of adaptive value, predetermined in the germ plasm and in their initial stages independent of natural selection;

(6) The law of *polyphyly;* i. e., the normal occurrence of many related lines of descent, derived eventually from a common stock, but coexisting throughout great periods of time.

Osborn himself was under no delusion as to the lack of enthusiasm with which his writings on the theory of evolution were received in many quarters. In his autobiography he writes (p. 73):

". . . . it is important to warn the investigator that the assemblage of new facts and observations and what may be called the first line of conclusions or generalizations drawn from these observations makes much more rapid headway in science than the contemporary or final assemblage of new principles. In my own experience, the *facts and observations* I have assembled, with the aid of my colleagues, have gained world-wide acceptance and the subject of vertebrate palaeontology, with the contemporary work of other investigators along the same lines as my own, has become a matter of widespread knowledge and interest, whereas a half-century ago it reached only the scientific and cultured classes. . . .

[21] Science, Nov. 15, 1935, Vol. 82, No. 2133, pp. 452-454.

"The inductive line of investigation for the *new biologic principles* gained by extensive-intensive research advances much more slowly because it has to meet and contend with other principles strongly entrenched in the minds of observers in different fields of biologic research. . . .

"The new principles which I have enunciated from fifty-two years of palaeontological research—of Continuity, of Adaptive Rectigradation, of Allometrons—as well as the theory of Tetraplasy and Tetrakinesis, *have, so far as I know, gained no acceptance in the current realm of either biologic or palaeontologic thought.* The environmental principle of Adaptive Radiation, of polyphylogenetic evolution and of harmonic classification has, on the other hand, gained wide acceptance among palaeontologists and some measure of acceptance among zoologists.

"One need not be impatient; *if new principles are sound they will finally gain universal acceptance; if unsound, the less widely they are accepted the better.*"

RETROSPECT

In the midst of his diverse administrative labors Osborn found time to produce an enormous amount of manuscript for the printers, ranging over a wide variety of subjects. With the assistance of Miss Jannette M. Lucas and Miss Ruth Tyler, a critical examination of the nine hundred and forty-four entries in his complete bibliography was undertaken, with a view of measuring the size of his output in the six successive decades of his productive career. After careful consideration, corrections were made for the work of collaborators and assistants, for bibliographies and other clerical matter. Even with all these deductions he was the author of some 11,464 printed pages, not counting the still unpublished Volume II of the Proboscidea monograph, which would bring the total up to well over 12,000.

The net total outputs for the successive periods were as follows:

Periods	1878-88	1889-98	1899-08	1909-18	1919-28	1929-38
Net total pages.....	397	1,391	1,188	2,695	3,232	3,197 est.

Grand total 12,100.

These figures should, of course, be used with caution, since they show primarily not when the material listed was produced but when it was published. For example, by far the greater part of the copy for the great two-volume memoir on the Proboscidea was prepared by him before 1931, but owing to inevitable delays the work was not published until several years after his death. Moreover, a page containing an analysis of the phyla of the Mastodontidae cost the author far more time and effort than did a dictated page of the president's annual report.

Turning to the relative abundance of different subjects in successive periods, we have the following comparison expressed in net number of pages published, omitting miscellaneous matter, newspaper reports and the like.

	1878-88	1889-98	1899-08	1909-18	1919-28	1929-38	Net Totals
Palaeontology	237	709	920	1,205	547	2,552	6,170
Psychology	34	0	0	0	0	0	34
Neurology	77	2	0	0	0	0	79
Embryology	45	0	4	0	0	0	49
Heredity	1	104	0	0	29	9	143
Biography	3	76	20	219	771	143	1,232
Education	0	85	64	68	422	17	656
Administration	0	51	59	445	451	335	1,341
Evolution (Theory)	0	338	90	155	105	108	796
Anthropology	0	26	5	580	492	31	1,134
Conservation	0	0	26	23	44	0	93
Science and Religion	0	0	0	0	371	2	373
	397	1,391	1,188	2,695	3,232	3,197	12,100

From the figures above it will be seen that Osborn's strictly palaeontological writings constituted more than fifty per cent of his entire output. His contributions to psychology, neurology and embryology were relatively brief and practically limited to the first decade.

His publications under administration do not by any means measure his productive labors in that field, in which an enormous correspondence is on file and in which much was accomplished by personal contacts.

His publications listed under biography number about one hundred and six and range from brief single-page obituary notices to extensive works on Leidy, Cope, and Wallace. Especially in the longer biographies he endeavored to follow the historical development of the main ideas and principles that had been put forth by great men, who for the most part had been his own friends.

The Roosevelt Memorial, a building erected by the State of New York in memory of his lifelong friend, Theodore Roosevelt, may fairly be counted as Osborn's supreme achievement in biography, for although the first suggestion came from others, it was he, first as Chairman of the Roosevelt Memorial Commission, then as Chairman of the Board of Trustees of the New York State Roosevelt Memorial, who guided and inspired the entire project until it stood practically completed in the year of his death (1935). It is indeed a magnificent memorial in stone, bronze and mural painting, setting forth the life of a great naturalist, statesman and patriot.

The Roosevelt Memorial together with the American Museum of Natural History, with which it is articulated and integrated, affords a capital example of "creative evolution," the favorite principle of Osborn's later decades. As he watched wing after wing of the Museum itself and finally the capstone of the entire structure, the Roosevelt Memorial, rising under his wand, it is perhaps not surprising that he stressed the fact that there was something utterly new in the completed organization that was not present in any of its parts. He had repeatedly banished Chance from his cosmos and he therefore could not have granted that unpredictable combinations of diverse causal series had played a constant part in the development either of the Museum, or of the Roosevelt Memorial, or in his own history, or in the evolution of every organism of present and past ages.

"This discovery," he writes,[22] "of the firm and undeviating order with which palaeontology replaces all the chance explana-

[22] Address Delivered on the Occasion of the Dedication of the New Museum Building, 29 December, 1925: The Origin of Species, 1859-1925. Peabody Museum of Natural History, Bull. 1, no. 1, pp. 25-38.

tions of adaptation from Empedocles to Darwin is the supreme service which palaeontologic research renders to biology." But he does not tell us how, if there be no real freedom in the universe, the "creative evolution" of new effects becomes possible. In this connection, his brother, William Church Osborn, in response to an inquiry from the Reverend Henry S. Coffin, regarding the "long background which explains a man's view," attributed Henry Fairfield Osborn's view on creative evolution to the deeply religious nature of his mother and the training which she gave him in his formative years.

"He did not have a metaphysical mind," wrote his brother (*in litteris,* Dec. 3, 1935), "or a natural spirit of destructive skepticism. The whole tenor of his nature was towards creation and one of his last views on evolution was, if I correctly understood it, a belief that life contains within itself a creative power which leads it to adapt itself to its external surroundings and to create such new forms for itself as are required for its existence. This seems to me to be a distinctly religious conception as opposed to the mechanistic theory of straight survivalism by the selection of the fittest under the long processes of trial and error. I know that I once quoted to him a line from Lowell's 'Sir Launfal' which seemed to impress him deeply. I believe it is as follows:

> "'Every clod feels a stir of might,
> An instinct within it which reaches and towers
> And, groping blindly above it for light,
> Climbs to a soul in grass and flowers.'

"I do not think he ever allowed what he did know to overcome his feeling for the vast reaches that he did not know but I think he carried his faith in the creative power over into the latter and regarded the subjects in which his life was spent as merely steps or processes in a great forwarding of creation."

Those of his scientific colleagues who were perhaps somewhat irritated by his numerous press interviews and publications dealing with religion and science failed to realize that he, as the

disciple of Dr. McCosh, consistently regarded evolution as an expression of the "firm and undeviating order" conceived by the divine creative mind.

That he was a deist, at least according to his own understanding of the term, there is no doubt. On the other hand, there is abundant evidence that he was not a Fundamentalist. He professed to be a Christian and although he was silent on doctrinal points, he lived in reverence to his God and in service to man.

It is not, however, as a harmonizer of religion and science that Osborn was acclaimed by his contemporaries and will be especially remembered by his successors. It is not even as the author of controversial writings in anthropology or of innumerable reviews and addresses on the theory of evolution. Republics are traditionally ungrateful and in the great universities that he served so well, his name, although engraved on the roster of their immortals, will become dim with time. Even the American Museum of Natural History may eventually forget that long ago he was its master builder. But to the future students of vertebrate palaeontology (and there will be many such if civilization endures) his numerous memoirs on fossil mammals will be an imperishable source.

And not the least of the great endowments that the world owes to Henry Fairfield Osborn is that, with the loyal aid of his colleagues and assistants, he founded and established a single great school of vertebrate palaeontology located in three harmoniously coöperating centers, Princeton University, Columbia University and the American Museum of Natural History; that in place of discord he left peace among the palaeontologists of America, many of whom are proud to have been either his students or students of his students.

A man of the highest ideals and standards as teacher, investigator, citizen; great in leadership and administration, a man of scrupulous honor, of disarming sincerity and fairness, generously giving credit to all who aided him, respecting the principle of academic freedom in his assistants even when they differed sharply from him. He was gentle, good humored, and consistently adored by his parents, brothers, wife, children and

grandchildren. His friends delighted in his society and his colleagues testified their admiration by awarding him the highest medals and honors in the leading scientific organizations of the world. And at the end of this crowded, happy life he died seated at his desk at Castle Rock, as he was preparing for another day's work on the Roosevelt Memorial.

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ALBERT ABRAHAM MICHELSON

1852–1931

BY

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It will probably be generally agreed that the three American physicists whose work has been most epoch-making and whose names are most certain to be frequently heard wherever and whenever in future years the story of physics is told are Benjamin Franklin, Josiah Willard Gibbs, and Albert A. Michelson. And yet the three have almost no characteristics in common. Franklin lives as a physicist because, dilettante though he is sometimes called, mere qualitative interpreter though he actually was, yet it was he who with altogether amazing insight laid the real foundations on which the whole superstructure of electrical theory and interpretation has been erected. Gibbs lives because, profound scholar, matchless analyst that he was, he did for statistical mechanics and for thermodynamics what Laplace did for celestial mechanics and Maxwell did for electrodynamics, namely, made his field a well-nigh finished theoretical structure. Michelson, pure experimentalist, designer of instruments, refiner of techniques, lives because in the field of optics he drove the refinement of measurement to its limits and by so doing showed a skeptical world what far-reaching consequences can follow from that sort of a process, and what new vistas of knowledge can be opened up by it. It was a lesson the world had to learn. The results of learning it are reflected today in the extraordinary recent discoveries in the field of electronics, of radioactivity, of vitamins, of hormones, of nuclear structure, etc. All these fields owe a large debt to Michelson, the pioneer in the art of measurement of extraordinarily minute quantities and effects.

I. MICHELSON, THE MAN

Of all those who take their place in the company of the immortals the world is intensely interested in knowing not merely their accomplishments, but how they got started in the line they followed and what manner of men they were. Not very much of this can be learned from Michelson's writings, much less than from those of most equally famous men, and for that

reason this memoir must take on, to an unusual degree, the character of a personal narrative.

The total volume of Michelson's published work is very small. In an active life as a physicist extending from the age of twenty-five to that of seventy-nine, he wrote two small books that are found in all libraries. The first is entitled "Light Waves and Their Uses", University of Chicago Press, 1903. It represents his Lowell Lectures delivered in 1899. The second is "Studies in Optics", University of Chicago Press, 1927, and consists of a condensed summary of his major researches. These books contain some revealing and quotable passages which are used by Hale [1] and Lemon [2] in interesting and informing articles on Michelson.

His bibliography of scientific papers contains but seventy-eight titles all told, and many of these are abstracts and most of them are quite short articles. Not a few of them are reprintings of the same article in different journals and different languages. One's knowledge of Michelson must be gained, then, more from what he did than from what he said. Also, one must call upon the testimony of those who, like the present writer, lived and worked side by side with him for a quarter-century and more. That testimony will, I think, be unanimous in the judgment that his most outstanding characteristic was his extraordinary honesty, his abhorrence alike of careless, inexact, ambiguous statement, as well as of all deception and misstatement. His was a remarkably clean-cut mind, which left little room for adjustment and compromise.

Everyone knows that he was a remarkably accurate and dependable scientific observer, but not everyone knows how well he succeeded in doing what every scientist should do but which many do not succeed in doing, namely, in carrying over that exceptional dependability into all his relations with his fellows. At one time when he had become the subject of some criticism, one of the critics came to me and to another of Michelson's associates, and asked whether these criticisms were well founded. Our joint reply was, "Don't ask us; ask him. He'll tell you."

[1] Astronomical Society of the Pacific *XLIII*, 175, 1931.
[2] The American Physics Teacher *IV*, 1, 1936.

The critic took the advice and returned with the statement, "He certainly told me." His whole effort seemed to be to see and to state things just as they were. The writer played tennis with him for twenty-five years, and watched him call balls. In doing so he was always just and correct, but never generous either to himself or to his opponent. He did not even fool himself, as most of us do, in the analysis of his own motives. Even when those motives were incorrect from the standpoint of his associates, he stated them frankly and with amazing honesty, in one remarkable instance apologizing for his attitude and expressing regret that he was "built that way." Almost anyone else would have rationalized his conduct into a virtue, and lied both to himself and his critics, as we are now seeing done so continuously and so deplorably in our political life.

I am quite certain that the last thing Professor Michelson would desire would be to have this memoir made merely an eulogy. I am therefore endeavoring to give first as correct a picture as I can of the man as I knew him through more than twenty-five years of intimate association.

If his most outstanding characteristic was his honesty, his second most notable characteristic was the singleness, simplicity, and clarity of his objective—an objective from which he let nothing divert him however great the pressure. He was an intense individualist, he knew what he wanted to do, he had confidence in his ability to do it, and he refused to let anything divert him from it, no matter what other interests had to be sacrificed, or who stood in the way. The result was that he made no pretense of keeping widely informed outside his own field. Indeed, he was always depreciating his own knowledge even of the field of physics, despite the fact that he was quite fond of telling the incident of how he once told G. Stanley Hall that if he wanted to keep a first rate physicist like himself at Clark University he would have to treat him like a first rate physicist.

There is no doubt, too, that in fields other than physics his reactions were sometimes hasty and occasionally purely emotional. This was well illustrated at the time of the blowing up of the Maine. President Harper had asked the historian Van Holst and the ex-navy man and scientist Michelson to address

the assembly of the students at the University of Chicago upon the situation that had been created by that incident. Van Holst counselled caution, delay, and painstaking investigation before any action was taken or even thought of. Michelson was for declaring war at once and immediately taking drastic steps.

But the best illustration of his singleness of purpose is furnished by the early history of the Ryerson Laboratory. From a scientific point of view this consisted very largely of the history of Michelson's individual contributions. Some of the personal sides of that history are worth telling here for the light they throw on Michelson, the man. I first met him at the exercises connected with the dedication of the laboratory at the spring convocation of the year 1894. He had been the commencement speaker, and in his address had emphasized the significance of refinement of measurement for the progress of science—a significance which subsequent years have shown to be vastly greater than even he foresaw, inspired prophet though he was. Incidentally, in that address he used another's words, I think Kelvin's, as to the unlikelihood of future discoveries coming from other work than that involving the sixth place of decimals. He afterwards upbraided himself unmercifully to me for ever having done so.

At the dinner which he gave in the evening of that day to some fifty visiting physicists he was rallied for introducing personal charm as an attribute of the coming six-decimal-place physicist, for with his jet black hair, his attractive hazel eyes, his faultless attire, and his elegant and dignified bearing, he made a striking figure, though his height was not over five feet seven or eight. The next day I attended his first lecture to the group of some six or seven graduate students, who were there for the work of the summer quarter at the University of Chicago. He was himself the graduate department, giving the only graduate course. The impression that was made upon me by that course and by his presentation of the "visibility curves" which had recently been obtained—curves which enabled one by the aid of skillful observing and more skillful analysis to read for the first time the fine structure of spectral lines far beyond the power of

any instrument to reveal it directly to the eye—was memorable. This course, along with about two conversations which I had with him when he came to my room to see how I was getting along with my measurements on the polarization of the light emitted by incandescent surfaces, impressed me with the fact that I was in the presence of one who had a far deeper understanding of optics than any one I had thus far met. Elegance of observational technique, elegance of analysis, elegance of presentation,—these were, I think, the impressions made on all of us younger men who had a chance to see Michelson's experimental work and hear him present it. He always lectured twice a week and quizzed once on the ground covered in the two preceding lectures, and he could cover more ground in an hour than any lecturer I ever heard. The two texts which he relied upon mostly for the material of his lectures, when he was not presenting his own work, were Mascart and Joubert's "Electricity and Magnetism" and Rayleigh's "Sound." The material covered in his lectures embraced the whole fields of electricity, sound, and optics. He did nothing with thermodynamics, to which he had a certain aversion, probably because of its extreme generality and lack of concreteness—I think he greatly underestimated its value and its accomplishment—and while he was sympathetic with the kinetic theory and molecular physics, he incorporated little of this field into his courses. Also, the newer physics,—radioactivity, electronics, and quantum theory—were largely outside the field of his interest, though of the Zeeman effect he made a careful study, both experimental and theoretical, and when X-rays were discovered he stepped aside from his routine, as nearly everyone else did, to try to contribute something to the elucidation of the source and nature of the new phenomenon.

It was probably fortunate for me in my very limited relations with him as pupil (summer of 1894, only) and my long continued relations with him as a subordinate member of his staff, that I had my own problems in which he only assisted me incidentally, and that I was never either his immediate assistant or his collaborator; for at least in those earlier years Professor

Michelson's attempts in Chicago at collaborative work, either with staff members or with graduate students, did not in general turn out very well either from his own point of view or from that of the collaborator. In the case of both of the two staff members with whom in the nineties he tried to do some work in common, difficulties arose, and when one of them left about 1900 he assured me that my "turn would come next." And yet during the next twenty-one years during which I was with him I could not have asked for more considerate and courteous treatment than I uniformly received from Professor Michelson. It is true that on two occasions I thought he was wrong and told him so, but his explanations were so completely straightforward and disarming that I could only laugh and tell him that he was the most honest man I had ever met.

His relations with graduate students during the ten years from 1894 to 1904 tell much the same story. He assigned but few thesis subjects and no small proportion of these turned out none too happily. I think it was in 1905 that he called me to his office and said, "If you can find some other way to handle it I don't want to bother any more with this thesis business. What these graduate students always do with my problems, if I turn them over to them, is either to spoil the problem for me because they haven't the capacity to handle it as I want it handled, and yet they make it impossible for me to discharge them and do the problem myself; or else, on the other hand, they get good results and at once begin to think the problem is theirs instead of mine, when in fact the knowing of what kind of a problem it is worth while to attack is in general more important than the mere carrying out of the necessary steps. So I prefer not to bother with graduate students' theses any longer. I will hire my own assistant by the month, a man who will not think I owe him anything further than to see that he gets his monthly check. You take care of the graduate students in any way you see fit and I'll be your debtor forever." From that time on Professor Michelson assigned very few, if any, thesis problems. And this decision was the correct one for him to make, for he gauged his own qualities and capacities cor-

rectly. He knew what he was best fitted to do, and he did not let anything or anybody deter him from that course. He took no part in general university administrative or instructional problems outside the department of physics. I never saw him in a faculty meeting despite the fact that, in the early days of the University of Chicago, faculty meetings were very important affairs where university policies were very thoroughly threshed out.

His regular departmental routine was as follows: He met his class of graduate students regularly three times a week for two lectures and one quiz. All graduate students took his courses when they were ready for them. His lectures were carefully prepared. They were very condensed, with most of the details left for the students to work out. They were considered hard courses. He worked every day with his personal assistant, and often with the mechanician, in his research laboratory, and about four o'clock he regularly went over to the Quadrangle Club to play tennis or billiards, at both of which he was quite proficient. His evenings he spent at home; for his life with his second wife, Edna Stanton (married in 1900), and their three daughters was an altogether happy one, although his earlier marriage had not in the end turned out successfully.

The foregoing characteristics explain, perhaps, the fact that during his earlier years Michelson acquired the reputation of being somewhat unapproachable, difficult, dictatorial, even inconsiderate. If he possessed these qualities in the earlier part of his life he certainly lost them in the later part, for the mellowing effect of his later years was particularly noteworthy. It was commented upon by all his intimates, as well as by his students. It is shown, too, by the fact that his first wife, from whom he was divorced in 1897 and who was later Mrs. Margaret Hemingway Shepperd, and by whom he had three children, one daughter and two sons (one of whom is at present an ethnologist in the Bureau of American Ethnology of the Smithsonian Institution) wrote me in 1932 that before his death he sent his lawyer to her to ask her forgiveness for any suffering he might have caused her.

II. MICHELSON'S PARENTAGE, CHILDHOOD, AND YOUTH

Albert Abraham Michelson was born at Strelno, then in Germany, a small town near the frontier of Poland, on December 19, 1852. His mother, Rosalie Przlubska, was the daughter of a well known physician, and was left motherless in early youth. In her various duties for the household she met, fell in love with, and married the proprietor of a dry goods shop, a man of forty years, Samuel Michelson. She was then but eighteen years of age. Before 1855 two children had been born, a boy and a girl. Because of the troublous times existing in that year in Poland, they decided to come to America where Samuel Michelson had a sister living in California. They took passage via Panama to San Francisco, and then moved on to the lovely little mountain town in Calaveras County called Murphy's Camp, described by Bret Harte in one of his stories, where Albert's early childhood was spent. The only bits of information I have been able to find regarding childhood influences are contained in the following excerpts from letters to me from the members of his family.

The first Mrs. Michelson writes: "In those mining camps there were many cultivated men seeking a fortune, among them a fine violinist who taught little Albert the violin. In 1886, with Albert Abraham Michelson and our three children, we visited Murphy's Camp and met many of the old miners, but they had no stories to tell me except the love that the child (Albert) had for the violin." Though not a finished musician, Michelson continued to play the violin throughout his life.

The following excerpt from a letter written me by A. A. Michelson's famous author sister, Miriam Michelson, shows how early and how completely the whole Michelson family had broken away from the religion of their race. "Both Albert Michelson's father and mother were born of Jewish parents, yet I should not say that ours was a religious family. I had no religious training whatever. Nor can I recall a religious discussion among us, nor a religious inhibition or compulsion. And I believe this unorthodox viewpoint would have been the case

with both parents and children no matter what religious belief the former might have inherited." Neither in aspect nor characteristic did Michelson ever reveal any racial penchants or prejudices. He apparently had no feelings whatever of that sort.

A third quotation from a letter to me from his son, Truman Michelson, may have a little interest. It reads, "A fact not usually known is that my father was a Mason, enrolled in Washington Lodge N 21 L and AM. He resigned upon going to study in Europe."

As the young Michelson grew older he was sent to San Francisco to school, where "he developed few companionships," but where he lived for several years with the family of the principal of the high school, who later paid him three dollars a month—his first earnings—to keep the physical instruments in order. When Albert was sixteen he went back to the home of his father, who by that time had moved his drygoods business to Virginia City, Nevada, famous because of the history of the Comstock lode and more famous because of the immortality which Mark Twain gave it. The elder Michelson wished his son Albert to enter the Navy, so when, the next year, there was a vacancy in Nevada's quota, Albert took the examination for congressional appointment to Annapolis. He tied with another boy, who through influence got the appointment while Michelson was made alternate, but also given, by the local Congressman, a personal letter to President Grant which it was hoped might enable him to get one of the ten special Presidential appointments at large to the Academy. He obtained his interview with the President, who, however, informed him that he had already exhausted his ten appointments at large. Nevertheless, one of the President's Naval Aids told young Michelson to go over to Annapolis, since there was still a chance of a vacancy through the failure of one of the new appointees who had not yet passed his examination. After three days of waiting at Annapolis this hope failed, and Michelson was just starting back for Washington to try again with the President when the Commandant sent a messenger after him and informed him that the President had given him "an appointment at large."

Since this was the eleventh such appointment Michelson always maintained that his career was started by an illegal act.

His record at the Naval Academy does not appear to have been in any way notable. His first wife writes that "it was not brilliant." The records at the Naval Academy contain merely the following entries:

Ensign A. A. Michelson, U. S. Navy. Born in Poland, December 19, 1852. Died May 9, 1931. Came to U. S. in 1854.
Appointed *Cadet Midshipman* at Naval Academy June 29, 1869.
May 31, 1873, detached from Naval Academy to await orders.
September 18, 1873, ordered to the "Monongahela."
October 31, 1873, detached from "Monongahela."
December 12, 1873, ordered to "Minnesota."
December 23, 1873, transferred to "Roanoke."
August 19, 1874, transferred to North Atlantic Squadron, and joined the "Colorado." Transferred to "Worcester."
October 14, 1874, detached from "Worcester" and ordered to appear before Examining Board.
July 16, 1874, promoted to Ensign.
December 15, 1875, ordered to Naval Academy.
May 22, 1877, ordered to "Constellation" (practice ship).
September 18, 1877, resumed duties Naval Academy. Became teacher and physicist in the capacity of Professor at the U. S. Naval Academy.

The first Mrs. Michelson, however, adds to this mere skeleton the following facts: In the course of his ship cruises young Michelson was one day in London visiting Dickens' recently garlanded tomb in Westminster. The striking appearance of the young naval cadet made a vivid impression upon the young American girl, Margaret McLean Hemingway, who happened to be standing opposite him with her parents. Miss Hemingway was the niece of the wife of Admiral Sampson, who shortly after was detailed to Annapolis as Professor of Physics in the Naval Academy. In December, 1875, Michelson was sent back to the Naval Academy as instructor in physics and chemistry. It was while Miss Hemingway was visiting her uncle and aunt that she at once recognized in the young instructor the midshipman cadet whose "brilliant eyes" had made such an impression upon her as they stood beside Dickens' tomb. They became engaged,

and on April 10, 1877, when she was 18 and he 24, they were married at Mr. Hemingway's summer home in New Rochelle, N. Y. After a summer cruise with the naval cadets, young Michelson and his bride returned to his duties at the Naval Academy.

III. MICHELSON AND THE VELOCITY OF LIGHT

In November of that same year, 1877, while studying, for the purposes of a lecture, the three purely terrestrial determinations of the velocity of light that had thus far been made, one by Fizeau in 1849, one by Cornu in 1872, both by Fizeau's toothed-wheel method, and one in 1862 by Foucault, a slight but, for accuracy, a very vital modification of the Foucault method suggested itself to Michelson, which, to quote his own words, "dispenses with Foucault's concave mirror and permits the use of any distance."

Foucault, and, following him, Newcomb who with the aid of a relatively large congressional appropriation had for some time prior to any work by Michelson been working at a determination of the velocity of light by a modification of Foucault's method, had placed the rotating mirror between the lens and the mirror used to return the beam back to the rotating mirror. In order to get enough light this required the use of a relatively large rotating mirror, and in Foucault's case the distance between the two mirrors was only twenty meters. It is to be remembered, however, that Foucault's apparatus was designed to permit the insertion between the mirrors of a tube filled with water, for his primary purpose was to determine whether the speed of light was greater in air or in water, for this was the crucial problem of his day and this problem he successfully solved. It is true, however, that with his arrangement it was impossible to get enough light to enable the use of a large distance between the two mirrors. Also, when in 1872 Cornu went at a precise, absolute determination he studied carefully and elaborately Foucault's rotating mirror method, and discarded it because he could not extend the intermirror distance to more than thirty meters. Michelson, though only an inexperienced youth of twenty-four, with that quick insight into the vital

elements of an experimental problem which was characteristic of all his design work, saw what other scientists of the highest repute who had studied the rotating mirror method had failed to see, namely, that by simply placing the point (or line) source at the principal focus of the lens so that the beam went out from the lens as a parallel bundle of rays which could be returned on itself as a parallel beam by a plane mirror *placed at any desired distance*, and then using a *small*, rotating mirror placed just in front of the point source, he could use any distance that he wished without any loss of light. This would make Foucault's rotating mirror method altogether comparable with Fizeau's rotating toothed-wheel method for an absolute measurement. Between November, 1877, and March, 1878, he built at an expense of ten dollars, a rotating mirror and using with it a lens which he found in the physics lecture equipment at Annapolis he obtained a displacement of 5 mm., while Foucault had had a displacement of .8 mm. Michelson wrote these results to the editor of the American Journal of Science. This letter takes up half a page of the May issue of 1878, and is Michelson's first publication. At the suggestion of his wife, her father in July, '78, gave him $2,000 for improving the precision of the method, and he was presently using a distance of 700 meters, instead of 20, and getting 133 mm. deflection. As a result of all his early determinations by this technique he obtained the value 299,895 km. per second, a value which he regarded as correct to one part in 10,000.

This problem, with which his career began, was also the one with which it closed. Four years before his death, at the age of seventy-five, he published in the Astrophysical Journal *65*, 1-14, 1927, the final mean of the results of his measurement made by sending a beam from Mt. Wilson to Mt. San Antonio, California, and back, 35 km. distant. The method now used, however, is essentially a combination of Fizeau's method and of Foucault's, since now Fizeau's rotating toothed-wheel is replaced by a rotating octagonal mirror, and the time of double transit of the light is the time it takes one face of the octagon to rotate into the position of the adjacent face. The advantage of this rotating mirror arrangement is that the angle of the octagon

can be measured much more accurately than the mean distance between the teeth of the rotating wheel. He got from these measurements the value 299,796 km. per second.

Not content with this experiment because of the possible disturbance of the air-path between the two mountains, he spent the last four years of his life preparing for and redetermining near Santa Ana, California, this velocity by means of multiple reflections between the ends of an underground pipe 1600 meters long, 30 cm. in diameter, from which the air had been pumped out to such an extent as to make it possible to measure directly for the first time the velocity essentially in vacuo. The final mean result, published after his death, may be taken as 299,774, which is one part in 2500 less than the best mean of his earlier measurements, another illustration of the fact that even the best of us tend to overestimate the precision of our work. The introduction to this last paper was written by Michelson but ten days before he lost consciousness. As he wrote it it had the same title as the paper with which he began his career, "On a Method of Measuring the Velocity of Light."

Going back, now, to the beginning of his career: By the measurement made in 1879 at Annapolis, Michelson had sprung at the age of twenty-six, and while he was still "Ensign Michelson of the U. S. Navy," into international repute as a physicist. That he received immediate popular acknowledgment is shown by the following interesting quotation from a local Virginia City paper published May 15, 1879, less than a year after his first published determination of the velocity of light:

"Ensign A. A. Michelson, a son of S. Michelson, the dry goods merchant of this city, has aroused the attention of the scientific minds of the country by his remarkable discoveries in measuring the velocity of light. The N. Y. Times in an article says that 'it would seem that the scientific world of America is destined to be adorned with a new and brilliant name.' Ensign A. A. Michelson, a graduate of Annapolis, not yet 27 years old, is distinguishing himself by studies in the science of optics in measuring the velocity of light."

But the importance of Michelson's work on the velocity of light is not to be measured alone by the absolute determinations thus far considered. He began his work in the period in which

physicists were trying to obtain crucial tests to distinguish definitely, if possible, between a wave theory and a corpuscular theory of light. Indeed, Foucault's method had been designed and used by him in 1862 primarily for the sake of finding by direct measurement whether the velocity in air was greater or less than the velocity in water, and he actually interposed a tube of water, closed at the ends by plane parallel glass plates, between his lens and his concave reflector and showed that the velocity is less in water than in air, as the wave theory demanded. In 1884, after Michelson had become (in 1883) Professor of Physics at Case School of Applied Science at Cleveland, he repeated this experiment of Foucault's and checked not only the latter's qualitative result, but he now made it definitely quantitative. He showed, also, that the ratio of the velocities in air and water is equal to the index of refraction, as demanded by the wave theory. He then performed the same experiment with carbon disulphide and here found the ratio of the velocities 1.75 instead of 1.64, as expected from the index of refraction. Confident, however, in the accuracy of his measurement, he published his results in spite of the apparent contradiction. Lord Rayleigh later removed the contradiction by showing that in a highly dispersive medium like carbon disulphide the *group velocity* is less than that computed from the mean index of refraction, and that the theory was quantitatively in accord with Michelson's measurements. Michelson also tested directly the velocities of red and blue light and found the former 2% greater than the latter,—a result of much importance at that time for the theory of dispersion.

IV. MICHELSON AND THE INTERFEROMETER

However important his work on the velocity of light may have been, the permanence of Michelson's place in physics undoubtedly rests in largest measure upon his invention of the Michelson interferometer and what he accomplished with it. Claude Bernard says that "a good technique sometimes renders more service to science than the elaboration of highly theoretical speculations," and George Hale has often remarked to me that

"after all the progress of physics is written in the history of the development of new instrumental techniques." Be this as it may, the history of the interferometer shows how vitally theory and experiment cooperate in the progress of science.

Early in 1879 Michelson left the Naval Academy, where he had been instructor in physics and chemistry since December 15, 1875, and was then employed for a year at the office of the Nautical Almanac in Washington. He and his wife and their two young children then started for Europe and he spent two years studying at Berlin, at Heidelberg, and at the College de France. At Paris he acquired a good command of French and became well acquainted with the French physicists of that period, particularly Mascart and Cornu, the latter of whom had made by Fizeau's toothed-wheel method a very excellent determination of the velocity of light, the value of which then stood at 299,990, while Michelson's value was at that time 299,940. It is probable that it was his careful study here in Paris of Fizeau's work that got him started on his main lifework in interferometry. For it was as early as 1851 that Fizeau had made his remarkable experiment on the effect of moving water on the speed of light passing through it. The method consisted in bringing into interference two rays of light after their passage through parallel tubes in which water was driven with a high speed, in one tube in the direction of travel of the light and in the other tube against that direction. It was but a step from this Fizeau form of interferometer to the one used by Michelson in which the two components of the split beam of light are sent off in directions at right angles to each other and brought back by mirrors placed at right angles to each beam, to the original separating surface for the observation of the fringes. The complete control of the path of each beam, however, and the possibility of varying each path at will, or of introducing in either path materials of any sort whose optical properties it might be desired to study, gave it an extraordinary flexibility as a tool for making exceedingly refined measurements. It is not too much to say that Michelson spent a large part of his active life and did his most important work in devising new uses for this tool and carrying out researches of all sorts with its aid. In a sense, the tool had been

here for decades before him, but why had not its possibilities been seen and utilized? Michelson once told me that when he first set up such a device in Paris and told Cornu how he got the fringes, Cornu was skeptical until he put a piece of cardboard in one of the right-angular paths and saw the fringes instantly disappear.

Michelson's first use of his interferometer was for testing the relative velocity of the earth and the ether. It was while he was still in Europe at the age of twenty-eight that he made his first try at this epoch-making experiment. He reports it in the American Journal of Science *22*, 120 to 129, 1881. He had tried it first in Berlin, then moved to the Astrophysikalisches Observatorium at Potsdam. In his brief report he thanks Alexander Graham Bell for supplying the funds for the investigation, and is so confident of the correctness of the negative result obtained that he asserts that "The hypothesis of a stationary ether is thus shown to be incorrect."

It is not until 1886 and 1887 that this experiment, repeated at Case School of Applied Science with great care and refinement by Michelson and Morley, begins to take its place as the most famous and in many ways the most fundamentally significant experiment since the discovery of electromagnetic induction by Faraday in 1831. The special theory of relativity may be looked upon as essentially a generalization from it.

Only second to it in importance is the use which Michelson made of his interferometer, especially in the years 1887 to 1897, in proving, through his penetrating and very skillful study of the so-called visibility curves characteristic of different spectral lines, the great complexity of all save a very few of such lines. It was the analysis of these visibility curves which brought the discovery that the so-called red cadmium line of wave-length 6,438.472 angströms (an angström is a ten-millionth of a millimeter) is so extraordinarily monochromatic that it is desirable to express the length of the international standard meter in terms of it. At the invitation of the International Bureau of Weights and Measures, Professor Michelson, with his collaborator, Professor Morley, spent the year 1892 carrying through this very exacting undertaking, with the result that the number

of the foregoing wave lengths in this standard was determined as 1,555,165.5.

Under this head should also come the extraordinarily fine work on the application of interferometry to the measurement of the diameters of stars done at the request of Dr. George E. Hale and in collaboration with F. G. Pease at the Mount Wilson Observatory between 1920 and 1925, a measurement which, for the first time made possible the direct determination of a stellar diameter, and, to take but one example, fixed the diameter of Betelgeuse at 240,000,000 miles,—about a hundred times that of the sun. The essentials of the method had been published by Michelson as early as 1890.

V. MICHELSON, SPECTROSCOPY AND GEOPHYSICS

No event in Michelson's career showed the originality of his mind better than the echelon spectroscope, which appeared in 1898. Unlike the interferometer, the accomplishment of this instrument has not been large because of its very narrow spectroscopic range; but the idea of obtaining high resolution by using this particular means of getting into a spectrum of very high order was at the time so novel that spectroscopists the world over were surprised and delighted with it. It showed, too, how fundamental an understanding its author had of all the elements of correct spectroscopic design. Its appearance probably had much to do with stimulating the minds of Fabry and Perrot to attain high spectroscopic resolution by modification of this route, which has found application to a somewhat larger number of problems.

The attainment of high spectroscopic resolution had by this time (1900) become a major objective with Michelson, and it was perhaps because he thought he had about exhausted the possibilities of the interferometer and the echelon for this purpose that he turned his attention to the problem that gave him more trouble and at the same time filled his associates with more admiration for him than any of its predecessors had done, namely, the problem of ruling very high resolution gratings. He had thought he could build a machine in a few months, or

at most a few years, which would give him the desired resolution, but he spent the rest of his life without reaching the point at which he was willing to drop the problem. He often said he regretted that he ever got "this bear by the tail," but he would not let go, and in spite of endless discouragements at the end of about eight years of struggle he had produced a good six-inch grating containing 110,000 lines (resolving power is measured by the number of lines times the order of the spectrum), which was 50% better than the best otherwise produced at that time and in 1915 he produced both an 8-inch and a 10-inch, which are still "among the most powerful instruments of diffraction that the world possesses," although with the extraordinary developments of quantum and nuclear physics the problem has become so important for physics, astronomy, chemistry, and even biology that a considerable number of institutions are now hard at the grating problem.

One of the finest things that Michelson ever said was inspired by his baffling experiences with his grating machines. It reads:

"One comes to regard the machine as having a personality—I had almost said a feminine personality—requiring humoring, coaxing, cajoling, even threatening. But finally one realizes that the personality is that of an alert and skillful player in an intricate but fascinating game, who will take immediate advantage of the mistakes of his opponent, who 'springs' the most disconcerting surprises, who never leaves any result to chance, but who nevertheless plays fair, in strict accordance with the rules he knows, and makes no allowance if you do not. When *you* learn them, and play accordingly, the game progresses as it should."

The problem of the rigidity of the earth which Michelson, in collaboration with Gale, solved so magnificently in 1919 is unlike most of Michelson's work in that it was undertaken at the request of others, particularly the geologist, T. C. Chamberlin, who was intensely interested in knowing how rigid the earth as a whole is. If it acted throughout like a fluid, then the land would move just as does water under the influence of the moon and the sun, and there would be no water tides at all relative to the surface of the earth. Could not Michelson suggest and carry out a method of making an exact measurement? Michelson at once devised the following exceedingly simple and direct

method which, however, save for the "fringe system" of measurement had been suggested earlier, I think by Airy, though never actually tried. It consisted in burying about ten feet under ground two six-inch iron pipes about 500 feet long, one running east and west, the other north and south, with an observation chamber at the junction point. The pipes were filled half full of water. The variation in the levels of the water at the ends of the pipes, as the moon and sun periodically produced their miniature tides in the water in these two pipes, were accurately measured by the movement they produced in a simple optical fringe system formed between the top of the water and a surface rigidly attached to the earth. The amplitude of the movement was from 6 to 11 microns. If the earth were not at all rigid, as already stated, there would be no relative movement of the earth and water in the pipes at all, and hence no movement of the fringes. If the earth were completely rigid the movement could be exactly computed from the pull of the moon and the sun. The movement was actually about half of that computed for an immobile earth.

These results, published in the Astrophysical Journal by Michelson and Gale for 1919, undoubtedly give the best values yet obtained of the earth's rigidity, as well as of its viscosity. The difficult tidal computations were carried out by F. R. Moulton of the University of Chicago, and his staff.

VI. MICHELSON, THE ARTIST

This sketch would not be complete without an endeavor to appraise somewhat more fully the artistic side of Michelson's personality. In a sense I have already paid the highest possible tribute to his artistry in describing the refinement and exactness of his measurements and the perfection of the design of his instruments, for are not discrimination in the choice of tools and methods and exact adaptation of means to end of the very essence of real art? Michelson was incessantly trying to perfect his artistic techniques, practicing his tennis strokes, taking lessons to improve his billiard shots. His students were continually commenting upon the perfection of the circles which in his lectures he drew with such ease on the blackboard. Is it at all

strange, then, that he was interested in music and a good performer on the violin? The connection between his accurate analysis of spectral colors and his love of painting landscapes and seascapes is a little less obvious, but be that as it may, in Southern California, where he spent a considerable fraction of the last ten years of his life, he divided his time between scientific pursuits and painting expeditions to the beaches, arroyos, and the High Sierras. In the summer of 1925, when I came down from a week spent studying cosmic rays in Muir Lake under the brow of Mt. Whitney, I found Michelson all alone seated with his easel in a favorable spot on the porch of the little Lone Pine Inn painting the glorious view he had found there of the snow-capped Whitney. In other words, he not only had the skill of the artist but also the feelings of the artist. He wrote two papers in which he gave expression to these feelings; one written in 1906 is entitled "Form Analysis." In it he attempts a classification of symmetrical forms as they are found in nature. and expresses the delight he found in these discoveries. He made this analysis, however, rather as a recreation than as a serious study. The other paper, written in 1911, is on "The Metallic Coloring of Birds and Insects," and in it, while he has set himself the scientific objective of finding whether the irridescent colors found in birds and insects are due to pigmentation, interference, or metallic reflection, he gives expression to the feelings aroused in him by these beautiful color effects found in nature. Also in a paragraph from the Lowell Lectures he uses the following words:

"The aesthetic side of the subject is by no means the least attractive to me. I hope the day is near when a Ruskin will be found equal to the description of the beauties of coloring, the exquisite gradations of light and shade, and the intricate wonders of symmetrical forms and combinations which are encountered everywhere."

In 1928, a few years before Professor Michelson's death, a conference on the Michelson-Morley experiment was held at Pasadena. It was a distinguished gathering, with both Lorentz and Michelson having a part in the program. The latter was scheduled to make a final report on the repetition he had just

made on Mt. Wilson of this famous experiment. Roy Kennedy, a young physicist who had repeated this experiment at the Norman Bridge Laboratory in a new way and with much precision, preceded Michelson. With a generosity and courtesy altogether characteristic of him, Michelson rose and complimenting Kennedy enthusiastically upon the beauty and precision of his experiment said, "Your work, Dr. Kennedy, renders my own work quite superfluous. I should not have undertaken it had I known you were doing it so well." It was one of the finest tributes Michelson could have paid *to himself*.—A wonderful ending of a wonderful career, for this was Michelson's last public appearance.

The bibliography of his published papers, as prepared by Professors Monk and Lemon of the Ryerson Laboratory, is given below.

MICHELSON IN HIS SEVENTIES

BIBLIOGRAPHY OF PUBLICATIONS OF ALBERT ABRAHAM MICHELSON*

Books

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***Light Waves and Their Application,* Russian tr. (Mathesis, Odessa, 1912). Another translation was published in St. Petersburg in 1912.

Articles

Papers listed under the same number are identical except when distinguished by a sub-classification, in which case the nature of the differences is explained.

(*a*) "On a Method of Measuring the Velocity of Light," Am. J. Sci. (3) **15**, 394-95 (1878); (*b*) "Velocity of Light (Preliminary Announcement)," Nature **18**, 195 (1878), differs from (*a*) in the addition of a paragraph.

"Experimental Determination of the Velocity of Light," Proc. A. A. A. S. **27**, 71-7 (1878).

"Experimental Determination of the Velocity of Light": (*a*) Proc. A. A. A. S. **28**, 124-60 (1879); (*b*) Nature **21**, 94-6, 120-22, 226 (1879-80), gives corrected values of velocities; (*c*) Am. J. Sci. (3) **18**, 390-93 (1879), abstract.

"Experimental Determination of the Velocity of Light," U. S. Naut. Almanac Office, Astron. Papers **1**, Part III, 115-45 (1880).

"Modifications Suffered by Light in Passing Through a Very Narrow Slit," Phil. Soc. Wash., Bull. III, 119-20 (1878-80). An abstract, not made by Michelson, of a paper read by him before the Society; its substance is not included elsewhere in this bibliography.

"The Relative Motion of the Earth and the Luminiferous Ether," (*a*) Am. J. Sci. (3) **22**, 120-29 (1881); (*b*) Comptes rendus **94**, 520-23 (1882), a shortened account of (*a*) with a correction.

"Nouveau thermomètre très sensible, J. de Physique (2) **1**, 183-86 (1882); Séances de la Soc. de Physique, Paris, 66-9 (1882-83).

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* Acknowledgments are due to Ardis T. Monk for the accurate revision of this bibliography from one previously published in J. O. S. A. **18**, 151 (1929).

** These items are not verified.

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N. L. Britton

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OF

NATHANIEL LORD BRITTON

1859–1934

BY

E. D. MERRILL
With bibliography by
JOHN HENDLEY BARNHART

PRESENTED TO THE ACADEMY AT THE ANNUAL MEETING, 1938

NATHANIEL LORD BRITTON

1859–1934

BY E. D. MERRILL

To establish one's self as one of the outstanding productive botanists of his time is an achievement in itself, but it is quite another thing at the same time to develop in connection with one's active scientific work, an outstanding scientific institution planned to perpetuate and increase research in the field in which the individual was interested. Yet this is the record of accomplishment of the subject of this biographical memoir. As ably expressed by Doctor Marshall A. Howe:

"Opportunity and the man conjoined to make the career of Nathaniel Lord Britton a notable one. The City of New York, spacious and wealthy, was a fitting site for an institution to be devoted to the study of plant sciences and to the public display of plants and plant products of scientific, economic, and horticultural interest. Doctor Britton was the man of vision, energy, and resource, who, above all others, made the dream of a few a living reality. In a very large measure, it was his driving, vitalizing force that, within less than thirty-five years, converted raw materials into the New York Botanical Garden, one of the leading institutions of the kind in the world."

Born at New Dorp, Staten Island, now the Borough of Richmond of New York City, on January 15, 1859, Doctor Britton died at his home, 2965 Decatur Avenue, New York, June 25, 1934, in the seventy-sixth year of his age. He was the son of [Jasper] Alexander Hamilton Britton and Harriet Lord Turner, and it has been said that his parents hoped that he might follow a clerical career. The interest of a neighbor, John J. Crooke, an individual of considerable scientific attainments himself, led to young Britton's being sent to the School of Mines in Columbia College, where he was prepared for his future scientific career. Doubtless his early close association with the late Doctor Arthur Hollick, a classmate in the School of Mines, and the influence of Professor John Strong Newberry, Professor of Geology and Mineralogy, and an "old school" naturalist, had their effect on the shaping of Doctor Britton's future career.

It is probable that Doctor Britton was a descendant of James Britton who came to the American colonies from London in 1635, although it seems likely that the name was of French rather than of English origin. The name is not an uncommon one in the early records of New Jersey and New York. In any case William Britton settled on Staten Island about 1664. In 1695 Nathaniel Britton acquired title to the Obadiah Holmes property in New Dorp, Staten Island, and in 1925 Nathaniel Lord Britton and his wife Elizabeth Gertrude, deeded a part of this land, with the old Britton cottage, to the Staten Island Institute of Arts and Sciences to ensure its preservation as long as possible as an example of early colonial construction.

It is apparent that Doctor Britton's interest in botany developed at an early age, long before he entered college, for it is recorded that even as a child he knew the names of local plants in a way that seemed very mysterious to his parents. In any case, in spite of his collegiate training in geology and mining, the study of plants soon became his dominant interest.

In 1879, before he had reached his twenty-first birthday, he was graduated from the School of Mines of Columbia College with the degree of Engineer of Mines. At that time there was little formal instruction in botany in Columbia College, Professor Newberry giving lectures in both geology and botany. He had published numerous papers on fossil plants and a few on living ones, and naturally encouraged young Britton's botanical interests. Like many others who made their mark in botanical science in the days before the great development of laboratory technique so characteristic of modern botanical training, Dr. Britton's interest was a personal one because of his abiding interest in plants and in plant life. He was to a very considerable degree self-trained in his chosen career. He joined the Torrey Botanical Club in October, 1877, and in the year of his graduation his first extensive botanical paper appeared, this being "The flora of Richmond County, New York," in joint authorship with Arthur Hollick, although he had published shorter botanical notes in 1877 and 1878.

Following his graduation from the School of Mines he was appointed Assistant in Geology under Professor Newberry and

for the five succeeding years he also served as Botanist and Assistant Geologist on the Geological Survey of New Jersey. In 1881 his "Preliminary Catalogue of the Flora of New Jersey" appeared, this apparently serving as his thesis for the degree of Doctor of Philosophy which he received that year from Columbia College. In 1887 he received an appointment as Instructor in Botany and Geology, at Columbia. He even gave courses in zoology in 1887-88. At this time he largely took over Professor Newberry's botanical instruction. In 1890 he was appointed Adjunct Professor of Botany, and in 1891 Professor of Botany. On his appointment as Director-in-Chief of the newly established but as yet unorganized New York Botanical Garden in 1896 he became Professor of Botany Emeritus at the early age of thirty-seven years. Under arrangements perfected between the Garden and Columbia University, the Director of the Garden became, ex-officio, a Professor of Botany in the University.

Doctor Britton was married on August 27, 1885 to Elizabeth Gertrude Knight who materially assisted him in his botanical efforts because of her own abiding interest in botany. Starting with a keen interest in the local flora he gradually extended his field assembling data, perhaps unconsciously at first, but later with the definite motive of preparing the famous Britton and Brown "Illustrated Flora of the Northern United States and Canada" which appeared in three volumes 1896-98; there was a second edition in 1913, and this, forty years after its first publication, is still standard although, of course, revision is now needed. It was the first fully illustrated "Flora" on any part of North America. The preparation of such a work, an enormous task in itself, did not restrict the author's output of other papers, and during much of the period devoted to its preparation Doctor Britton served as editor of the Bulletin of the Torrey Botanical Club (1889-97), the pioneer American botanical periodical.

The illustrated flora project was the joint idea of Doctor Britton and Judge Addison Brown. Its preparation was made possible through funds provided by Judge Brown, but there is every reason to believe that the initial outlay was repaid by royalties from its sale. This reference work, although of course

more or less out of date, is still in demand, with steady annual sales of several hundred copies each year. While Doctor Britton devoted an enormous amount of time to its preparation, Judge Brown worked with him throughout the period of its preparation and publication.

Other outstanding major works include his "Manual of the Flora of the Northern States and Canada" (1901, ed. 2, 1905, ed. 3, 1907), "North American Trees" (1908, with J. A. Shafer), the "Flora of Bermuda" (1918), the "Bahama Flora" (1920, with C. F. Millspaugh), and "The Botany of Porto Rico and the Virgin Islands" (1923-30, with P. Wilson). His *magnum opus,* prepared in association with J. N. Rose, is "The Cactaceae", a four volume, copiously illustrated monograph of this most difficult plant family, published by the Carnegie Institution of Washington, 1919-23. In this work 124 genera and about 1,237 species of this perplexing family are described. No matter what future botanists may attempt or achieve in their consideration of this great family of plants, this Britton and Rose monograph forms a datum plane from which all future work must proceed.

Doctor Britton became interested in the flora of the West Indies at an early date, the first expedition being sent by the Garden in 1898, his own first field trip being in 1901. He participated personally in at least thirty botanical expeditions in the islands, for he was convinced that exploration of this region would be most fruitful. Most of this West Indian work was financed by him personally. In connection with the problem of financing field work he was most successful in developing cooperative expeditions whereby several institutions contributed to the expenses of field parties, the resulting collections being equitably divided among the cooperating units.

Throughout his productive life, Doctor Britton was a voluminous writer. In his earlier career, teaching duties, while of course taking time, were not allowed to restrict his productive activity, for it should be remembered that it was Britton who established and organized the Department of Botany in Columbia University. When he took charge he found very inadequate literary facilities, comprising not more than 1000 volumes. The

valuable herbarium established by Doctor John Torrey, rich in types and historical material, was in the greatest disorder, suffering from neglect, and in utterly inadequate quarters. He undertook, with the greatest zeal and energy, the task of correlating the scattered elements into one series of botanical specimens and the building up of a botanical library worthy of the University. Within a few years the herbarium was more than tripled in size and the botanical library was vastly increased. Later, when charged with very heavy organization and administrative duties at the New York Botanical Garden, we find the same situation to prevail; nothing was permitted unduly to interfere with his productive output in botany. One marvels at his energy, his ceaseless mental activity, and at the diverse productive results.

In view of the comprehensive bibliography appended to this paper there is little need to go into detail regarding Doctor Britton's publishing activities. The long list speaks for itself. Others unhampered by exacting demands on their time by social duties, political contacts, appeals for funds, dealings with architects, engineers, city and corporation officials, and a thousand and one details appertaining to the establishment, financing, and development of a great institution may look with equanimity on their productive output in publication. Yet one wonders how many of them could have continued to be productive had they been faced with the multitudinal problems, large and small, that Doctor Britton met day after day, week after week, month after month, year in and year out, and still find time to produce technical paper after technical paper. It takes a rare combination of qualities, and above all persistence, abiding faith in one's work, and deep and unfaltering interest in the daily task to produce results under such circumstances, and these qualities were Doctor Britton's to an eminent degree. He worked unceasingly himself and he expected his associates to do likewise. Above all he provided the opportunities for his associates to accomplish that in which they were individually interested through his organizing and administrative ability.

After all is said and done, in spite of Doctor Britton's notable contributions to botanical science, his greatest achievement was

undoubtedly the establishment and development of the New York Botanical Garden, a living monument to his memory. The institution was his in a very real sense, and to it he devoted his best efforts through much of his productive life.

Doctor Britton himself is the authority for the statement that the idea of establishing such an institution was due to a remark made by Mrs. Britton in 1888, when they were visiting the Royal Botanic Gardens at Kew, to the effect "Why couldn't we have something like this in New York?" On October 24, 1888, Mrs. Britton gave a description of Kew at a meeting of the Torrey Botanical Club, and at the next meeting of the Club a special committee was appointed, of which Doctor Britton was a member, to consider what might be done in reference to the establishment of a botanical garden in New York. On January 8, 1889, an appeal for such a garden, prepared by the committee, was adopted and ordered printed. How many potentially great projects stop here. The situation has been ably presented by Doctor H. H. Rusby.*

"Doctor Britton's accomplishment in the establishment of our Botanical Garden is not likely to fail of appreciation by future generations, but they might easily fail to appreciate the difficulties attending such a vast work on such insufficient resources. Looking back on the conditions that confronted the enterprise, they seem appalling, and the undertaking hopeless—yet—here is the garden, just pride of a nation! None of the enthusiastic botanical band, with the exception of Judge Brown, possessed enough means to justify even the starting of a subscription list, and but a very few of them had wealthy associates who might become interested. Again it was Doctor Britton who succeeded in inducing those few to initiate a campaign for funds. For years, the attempt persisted, but the work lagged and it was not until a determined group of women, led by Mrs. Britton, took to the warpath, that the minimum endowment of $250,000 was secured, and of this only the income might be used. Money for grading, road, path and bridge building, the location of lakes and the erection of buildings and conservatories, had still to be secured from the city administration."

Following the adoption of the committee report by the Torrey Botanical Club January 8, 1889, the consent of the Department

* Science n. s. 80: 108-111. 1934.

of Parks was secured providing for the establishment of the proposed garden could means for its maintenance be procured. The corporation known as "The New York Botanical Garden" was chartered by special enactment of the New York Legislature effective April 28, 1891. The list of incorporators included the names of forty-eight distinguished citizens of New York. The Act authorized and directed the Park Commissioners to set aside for the proposed garden an area not exceeding 250 acres in some park north of the Harlem River, if, within seven years, the corporation should procure by subscription an initial endowment of not less than $250,000.

About four years later, when the required amount had been subscribed, the Park Commission was requested to set aside 250 acres in Bronx Park as a garden site, and the Board of Estimate was requested to appropriate $500,000 for the erection of suitable buildings, as had been made mandatory by the Act of the state legislature. The officers of the first Board of Managers consisted of Cornelius Vanderbilt, President; Andrew Carnegie, Vice-president; J. Pierpont Morgan, Treasurer; and N. L. Britton, Secretary.

In New York City there are several quasi-city units of great cultural value to the community. They are in part supported by direct city appropriations, in part by income from their endowments, and by gifts from philanthropically minded citizens; all are under the control of their own governing boards. Already established and in operation on this principle were the Metropolitan Museum of Art, the American Museum of Natural History, the New York Public Library, and the Brooklyn Institute of Arts and Sciences. To this category was now added the New York Botanical Garden, another orphan, adopted by and in part supported by the City; and soon after this (in 1895) the New York Zoological Park.

Late in 1895 or early in 1896 an agreement was made with Columbia College whereby the herbarium and botanical library of that institution were to be deposited at the Garden, the facilities of the Garden to be made available to the faculty and advanced students of Columbia College. On June 17, 1896, Doctor Britton was formally appointed Director-in-Chief of the new

institution. The garden site, 250 acres, in Bronx Park had been set aside in July 1895, to be increased in 1915 to an area of nearly 400 acres.

The challenge that Doctor Britton met was 250 acres of raw undeveloped land, no roads, bridges, fences, or buildings, an assured endowment of $250,000 of which the income only could be used, no laboratories, library, or herbarium, no staff, but with the hope of some financial support from the city and the potential hope that citizens of means would by gift or bequest, help support the infant institution. How well he met the challenge may be briefly indicated by the statement that in the thirty-three years of his directorate he saw the Garden develop from an idea to a well developed tract of 400 acres, a commodious administrative building and museum, ample greenhouse and conservatory facilities, a great collection of books containing 43,500 bound volumes, one of the great botanical libraries of the world, a reference herbarium in excess of 1,700,000 specimens from all parts of the world, well equipped laboratories, an active and productive staff, a publishing institution sending its product to the ends of the world, its initial endowment of $250,000 increased to approximately $2,500,000, an auxiliary membership (annual, supporting, and life) of nearly 2000 individuals, and its annual city appropriation greatly increased. The end attained justified the faith of the moving spirit in the enterprise, but how many men, situated as Doctor Britton was in 1895, would have had the courage to accept the challenge, and had the faith that the desired end could be accomplished?

One marvels at the early and rapid expansion of this new institution, particularly when one considers the paucity of financial support in the early critical years. As expressed by Doctor Rusby, who also quotes the expression that in those early years every dollar of the Garden's funds was made to do the work of four: "This success, in carrying the garden through its lean early decades, could never have been accomplished but for the generous and self-denying support of as loyal a staff as has ever blessed any similar institution, and this devotion was in no small degree attributable to the director's personality." Explorations were initiated in various parts of North America, in the Philip-

pines, in the West Indies, and in South America. A well-rounded research program, not confined to the taxonomy of the flowering plants and cellular cryptogams was rapidly developed, including plant physiology, plant pathology, palaeobotany, and popular education. Manuals were prepared covering the floras of the northeastern United States, the southeastern States, and the Rocky Mountain region. Soon there appeared in the periodical field the "Journal" and the "Bulletin" of the New York Botanical Garden, a series of "Contributions," and the "Memoirs," somewhat later to be followed by the establishment of "Mycologia" and "Addisonia." As early as 1901 work on the preparation of the "North American Flora" was initiated, originally planned to be completed in thirty volumes. Intended as a descriptive flora to cover all known North American species of plants in all groups from Alaska and Greenland to the West Indies and Panama, seventy-four parts were published in Doctor Britton's lifetime. This extensive undertaking was due in large part to Doctor Britton's initiative, the work being done by members of the staff of the New York Botanical Garden, with important contributions from botanists in other institutions. As another indication of his keen insight into what is necessary in a publishing botanical institution he pioneered again in creating the position of bibliographer on the staff, the first position of the kind, it is believed, in any botanical institution.

Doctor Britton's personal interests in the development of the work of the Botanical Garden were particularly strong in the building up of the very comprehensive botanical library and the great reference herbarium. He did not limit the accessions to publications and material appertaining particularly to the problems or to the areas in which he was personally interested. He took an exceedingly catholic viewpoint, in his desire to make the conditions as to material and literature of equal value to all staff members regardless of their fields of special interest. To the library he presented all published material that was personally sent to him by correspondents from all over the world, and freely used his own personal funds and secured funds from various other sources to acquire important sets of plants and private

herbaria as they became available to enrich the reference collections, and books to enrich the library.

His breadth of view is illustrated by the plan originated and consummated by him of bringing together in one place the scattered herbaria and botanical literature in New York, at the Garden, including the historical Torrey herbarium and the general herbarium of Columbia University, that of Barnard College, and the herbarium of the American Museum of Natural History. His interest was not limited to the higher plants as witnessed by his fortunately consummated plans of acquiring for the Garden the very important Ellis and Everhart collection of fungi, and the magnificent Mitten collection of mosses, both collections being exceedingly rich in historical material and containing many thousands of types. In the library field he planned and consummated the purchase of all the botanical works in the library of the Conservatoire et Jardin botaniques de Genève that were duplicated when the very important DeCandolle library was presented to the latter institution, thus vastly increasing the library resources of the New York Botanical Garden.

Outside of the library and herbarium interests of Doctor Britton was the field of palaeobotany. He took advantage of the opportunity of broadening the field of the New York Botanical Garden by establishing palaeobotanical work at the Garden, fully realizing, as apparently others have not done, that palaeobotanical research can best be done in close association with a great reference herbarium. In developing this field he naturally turned to his early associate Doctor Arthur Hollick, giving him the opportunity of developing this special field of investigation at the Botanical Garden. In no other botanical institution in America, and in very few elsewhere, was the field of research so broadly planned and developed as under Doctor Britton's leadership at this, the youngest and yet one of the greatest, botanical gardens in the world.

Doctor Britton was naturally the recipient of many honors, but honors rested lightly on his shoulders. He was the recipient of the Sc.D. degree from Columbia University in 1904, and the LL.D. degree from the University of Pittsburgh in 1912. He was elected a Fellow of the American Academy of Arts and

Sciences in 1925, a member of the National Academy of Sciences in 1914, a member of the American Philosophical Society in 1928, and a foreign member of the Linnean Society of London in 1925. He served as Vice President of the American Association for the Advancement of Science in 1896, President of the Botanical Society of America in 1898 and in 1920, President of the New York Academy of Science in 1907, and was made honorary president of the International Desert Conservation League in 1930. He was chairman of the Scientific Survey of Porto Rico and the Virgin Islands from its organization, at his instigation, by the New York Academy of Sciences, until his death. In the latter part of 1934, a peak 3200 feet high in the Luquello National Park in Porto Rico was named Mount Britton in his honor, in appreciation of his many years of service in developing our biological and geological knowledge of Porto Rico.

Although slight in build and of frail physique, Doctor Britton was a man of tireless energy, quick to perceive and to execute what needed to be done, with a remarkably retentive memory, a highly developed faculty for order and dispatch, and with the gift of concise expression. He was an individual of pronounced ideas, and in the often acrid nomenclatural controversies of the last decade of the last century and the first decades of the present one, he was an outstanding champion of strict priority in publication and a strong exponent of the so-called "American" code of botanical nomenclature as contrasted to the international rules. As a result his own publications and most of those prepared by his associates in New York were issued under the "American" code. Many botanists frankly admit that certain provisions of the "American" code were superior to the original provisions of the International code. The two are now so measurably close, except on the two questions of conserved generic names and Latin diagnoses, that the acrid controversies of the productive years of Doctor Britton's botanical career are now but a memory. Differences of opinion could not be avoided between exponents of the conservative viewpoint in nomenclature and the progressive or liberal element, and Dr. Britton was a consistent liberal.

While Doctor Britton would be considered conservative in the

matter of delimiting species, in generic segregation he was extreme, rather than conservative, tending to separate genera on what many botanists consider to be slight characters. Essentially, genera and species being subjective concepts, rather than objective realities, no such thing as legislative authority, as to what shall constitute the limits of a genus or a species, is possible. Doctor Britton did not dictate to his associates and subordinates, but rather let each use his own judgment on the complex problem of what constitutes the limits of this or that major or minor group. Always an individual of strong convictions, never hesitating to express his own ideas, no matter whether others might be expected to agree with him or not, Doctor Britton continued his productive work regardless of some perhaps just, but some distinctly unjust criticism. He knew what he desired to accomplish and was eminently successful in devising ways and means of accomplishing his purpose. His great accomplishment in the establishment of the New York Botanical Garden in a great and essentially commercial city, was never more than a means to an end with him, for always first and foremost in his thoughts was botanical research, the means for making such research possible, and the publication of the results obtained.

As his own financial resources increased he liberally supported, by his own personal funds, those projects that appealed to him, particularly those associated with the New York Botanical Garden. On his death, indicating his abiding faith in the institutions he helped to organize and to develop through their years of struggle, he bequeathed one-half of his estate in varying amounts, to the New York Botanical Garden, the Torrey Botanical Club, the New York Academy of Sciences, the Staten Island Institute of Arts and Sciences, and Columbia University.

Doctor Britton's wife, Elizabeth Gertrude Britton, herself a botanist of note, died four months before him, and from this shock of separation Doctor Britton never fully recovered. There were no children. He was survived by a sister, Harriet Louise Britton, and a brother, Richard H. Britton, both of Great Kills, Staten Island.

Sixty-nine species and varieties of living and fossil plants have been dedicated to Doctor Britton, and fifteen plants and

one animal have been named for Mrs. Britton. In addition two species of plants have been named in honor of the two conjointly. The generic names *Brittonamra, Brittonastrum, Brittonella, Brittonrosea, Bryobrittonia*, and *Neobrittonia* perpetuate his name among botanists. Further to perpetuate his name the periodical "Brittonia" was established as an official serial of the New York Botanical Garden in 1931, devoted to those fields of botanical research in which Doctor Britton was personally interested. In 1935 the great reference herbarium of the New York Botanical Garden was officially designated as the "Britton Herbarium."

The New York Botanical Garden is a constant reminder of the energy, wisdom, scientific attainments, and organization and administrative ability of Nathaniel Lord Britton. His name is indelibly associated with the annals of botany of North and South America and the West Indies. No monument is necessary to perpetuate his name for of him, in association with the great institution he established, it may truly be said: *Si monumentum requiris, circumspice.*

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1912

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Cassia polyphylla. Many-leaved senna. Addisonia 1: 3, 4 *pl 2.* March 31, 1916.
Nopalea Auberi. Auber's nopal. Addisonia 1: 19, 20, *pl. 10.* March 31, 1916.
The natural vegetation of the Isle of Pines, Cuba. Jour. N. Y. Bot. Gard. 17: 64-71, *pl. 170.* June 22, 1916.
Exogonium microdactylum. Slender red exogonium. Addisonia 1: 33, 34, *pl. 17.* June 30, 1916.
Opuntia macrorhiza. Large-rooted prickly pear. Addisonia 1: 37, 38, *pl. 19.* June 30, 1916.
Bequest of Emil C. Bondy. Jour. N. Y. Bot. Gard. 17: 87. July 14, 1916.
Nelson Smith. Jour. N. Y. Bot. Gard. 17: 103, 104. August 5, 1916.
The vegetation of Anegada. Mem. N. Y. Bot. Gard. 6: 565-580. "August 31" [September 8] 1916.
The rose garden plans. Jour. N. Y. Bot. Gard. 17: 111-115, *pl. 175-177.* September 12, 1916.
The New York Botanical Garden and Columbia University. Columbia Univ. Quart. 18: 352-359. September, 1916.
Sisyrinchium Bermudiana. Bermuda Iris. Addisonia 1: 43, 44, *pl. 22.* September 30, 1916.
Anthurium grandifolium. Large-leaved tail-flower. Addisonia 1: 53, 54, *pl. 27.* September 30, 1916.
A white pine planting. Jour. N. Y. Bot. Gard. 17: 152-154. October 14, 1916.
Studies of West Indian plants.—VIII. Bull. Torrey Club 43: 441-469. October 20, 1916. Also as Contr. N. Y. Bot. Gard. no. 192.
A "Convention Garden." Jour. N. Y. Bot. Gard. 17: 172, 173. November 3, 1916.
Opuntia vulgaris. Tall South American prickly pear. Addisonia 1: 75, 76, *pl. 38.* December 30, 1916.
Echeveria australis. Southern echeveria. Addisonia 1: 79, 80, *pl. 40.* December 30, 1916.

1917

Studies of West Indian plants.—IX. Bull. Torrey Club **44**: 1-37. February 12, 1917. Also as Contr. N. Y. Bot. Gard. no 195

Further development of the New York Botanical Garden. Jour. N. Y. Bot. Gard. **18**: 29-31. February 27, 1917.

Peperomia obtusifolia. Blunt-leaved peperomia. Addisonia **2**: 19, 20, *pl. 50.* March 31, 1917.

Report of the Secretary and Director-in-Chief for the year 1916. Bull. N. Y. Bot. Gard. **9**: 293-311. April 10, 1917.

Instruction in gardening, in cooperation with the International Children's School Farm League. Jour. N. Y. Bot. Gard. **18**: 53-61, *pl. 192.* April 13, 1917.

Courses in gardening in cooperation with the International Children's School Farm League. Jour. N. Y. Bot. Gard. **18**: 95-98. June 11, 1917.

El genero Rynchospora Vahl, en Cuba. Mem. Soc. Cubana "Felipe Poey" **2**: 185-200. "September-December, 1916" [June. 1917]. Also as Contr. N. Y. Bot. Gard. no. 194.

Forests, hygiene and ethics. N. Y. Forestry $\mathbf{4}^2$: 24, 25, 32. July, 1917.

Selden's Everyman's garden every week. Torreya **14**: 128. July 17, 1917. (Review.)

Harrisia gracilis. Jamaica harrisia. Addisonia **2**: 41, 42, *pl. 61* September 29, 1917.

Harrisia Martini. Martin's harrisia. Addisonia **2**: 55, 56, *pl. 68.* September 29, 1917.

Piaropus azureus. Light-blue water-hyacinth. Addisonia **2**: 67, 68, *pl. 74.* December 31, 1917.

1918

The relatives of catalpa trees in the West Indies. Jour. N. Y. Bot. Gard. **19**: 6-9, *pl. 209.* "January" [February] 1918.

Flora of Bermuda (illustrated). i-ix, 1-585, *frontisp, f. 1-519.* February 28, 1918.

An undescribed Scirpus from California. Torreya **18**: 36, 37, *f. 1.* March 8, 1918.

Aronia atropurpurea. Purple-fruited choke-berry. Addisonia **3**: 1, 2, *pl. 81.* March 30, 1918.

Opuntia lasiacantha. Slender white-spined prickly pear. Addisonia **3**: 19, 20, *pl. 90.* March 30, 1918.

First grant from the income of the Charles Budd Robinson Fund. Jour. N. Y. Bot. Gard. **19**: 47, 48. April 1, 1918.

Report of the Secretary and Director-in-Chief for the year 1917. Bull. N. Y. Bot. Gard. **9**: 403-433. April 30, 1918.

The Herbaceous Grounds. Jour. N. Y. Bot. Gard. **19**: 77-79. April 30, 1918.

John Adolph Shafer. Jour. N. Y. Bot. Gard 19 97-99. May, 1918. Reprinted, in Spanish translation. in Mem. Soc Cubana "Felipe Poey" 3: 225-228. "1918" [January, 1919]

A red pine plantation. Jour. N. Y. Bot. Gard. 19 105. 106 May, 1918.

Spring inspection of grounds, buildings, and collections Jour. N. Y. Bot. Gard. 19: 106-108. May, 1918

Torrey Botanical Club reminiscences. Mem. Torrey Club 17: 24-28. June 10, 1918.

Aronia arbutifolia. Red-fruited choke-berry. Addisonia 3: 33, *pl. 97*. June 29, 1918.

The flora of the American Virgin Islands. Brooklyn Bot. Gard. Mem. 1: 19-118. July 6, 1918. Also as Contr. N. Y. Bot Gard. no. 203.

The botany and plant products of northern South America. Science II. 48: 156, 157. August 16, 1918. (Anonymous) Reprinted: An investigation of the flora of northern South America Bull. Pan. Am. Union 47: 232-234. August, 1918. Reprinted in Jour. N. Y. Bot. Gard. 19: 182-185 "August" [September] 1918

The School Garden Shelter House given by Mrs. Frederick Ferris Thompson. Jour. N. Y. Bot. Gard. 19: 179. 180, *pl. 215*. "August" [September] 1918.

James Alexander Scrymser. Jour. N. Y. Bot. Gard. 19: 180, 181. "August" [September] 1918.

Opuntia Opuntia. Eastern prickly pear. Addisonia 3: 49, 50, *pl. 50*. September 30, 1918.

Byron David Halsted. Jour. N. Y. Bot. Gard. 19: 221. "September" [October] 1918.

1919

Dr. Henry Allen Gleason, appointed first assistant. Jour. N. Y. Bot. Gard. 20: 39, 40. "January" [February] 1919.

Report of the Secretary and Director-in-Chief for the year 1918. Bull. N. Y. Bot. Gard. 10: 1-24. May 12, 1919.

The Cactaceae. Descriptions and illustrations of plants of the Cactus family. (With Joseph Nelson Rose.) Volume I. i-vii, 1-236, *f. 1-302; pl. 1-36*. June 21, 1919. Carnegie Institution of Washington, Publication no. 248, volume I. Reprinted as a supplement to the Journal of the Cactus and Succulent Society of America, vols. 3-6 (1931-34).

A large tulip tree struck by lightning. Jour. N. Y. Bot. Gard. 20: 160. August, 1919.

History of the Survey. N. Y. Acad. Sci. Survey Porto Rico 1: 1-10. September 26, 1919.

Addisonia: its progress and publication. Jour. N. Y. Bot. Gard. 20: 181, 182. "September" [October 7] 1919.

The Scientific Survey of Porto Rico and the Virgin Islands. Jour. N. Y. Bot. Gard. 20: 220, 221. November, 1919.

1920

Flora of the District of Columbia. Torreya **19**: 244-246. (Review.)
The wild pimento of Jamaica. Jour. N. Y. Bot. Gard. **21**: 38, 39. "February" [April 9] 1920.
About Paulownia trees. Jour N. Y. Bot. Gard. **21**: 72, 73. "April" [May] 1920.
The Bahama flora. (With Charles Frederick Millspaugh.) i-viii, 1-695. June 26, 1920.
Cephalanthus occidentalis. Button-bush. Addisonia **5**: 17, 18, *pl. 169*. June 30, 1920.
Report of the Secretary and Director-in-Chief for the year 1919. Bull. N. Y. Bot. Gard. **10**: 213-219. June 30, 1920.
A botanical expedition to Trinidad. Jour. N. Y. Bot. Gard. **21**: 101-118. "June" [July] 1920.
The Cactaceae. Descriptions and illustrations of plants of the Cactus family. (With Joseph Nelson Rose.) Volume II. i-vii, 1-239, *f. 1-305; pl. 1-40*. September 9, 1920. Carnegie Institution of Washington, Publication no. 248, volume II. Reprinted; pages 1-148 as a supplement to the Journal of the Cactus and Succulent Society of America, vols. **6-8** (1934-36), the remainder separately in March, 1937.
Descriptions of Cuban plants new to science. Mem. Torrey Club **16**: 57-118. September 13, 1920. Also as Contr. N. Y. Bot. Gard. no. 225.
Two new West Indian plants. Torreya **20**: 83, 84. September 20, 1920.
George W. Perkins. Jour. N. Y. Bot. Gard. **21**: 179. "September" [October 13] 1920.
Report by the Director-in-Chief upon a visit to botanical institutions in England. Jour. N. Y. Bot. Gard. **21**: 197-208. November, 1920.

1921

Investigation of the flora of northern South America. Science II. **53**: 29, 30. January 14, 1921.
William Harris. Jour. N. Y. Bot. Gard. **22**: 13, 14. "January" [February 12] 1921.
Francis Lynde Stetson. Jour. N. Y. Bot. Gard. **22**: 44, 45. "February" [March 12] 1921.
Fanny Bridgham Fund. Jour. N. Y. Bot. Gard. **22**: 45, 46. "February" [March 12] 1921.
The leafy spurge becoming a pest. Jour. N. Y. Bot. Gard. **22**: 73-75, *f. 1*. "April" 1921
Further botanical studies in Trinidad. Jour. N. Y. Bot. Gard. **22**: 93-102. "May" 1921.
Neoabbottia, a new cactus genus from Hispaniola. (With Joseph Nelson Rose.) Smithson. Misc. Coll. **729**: 1-6, *f. 1, 2; pl. 1-4*. June 15, 1921. Also as Smithson. Inst. Publ. no. 2651.

The Cacti of Trinidad. Bull. Dep Agr. Trin. Tobago **19**: 81-87. [June] 1921.

Stewardson Brown. Jour. N. Y. Bot. Gard. **22**. 110-112. June, 1921.

Report of the Secretary and Director-in-Chief for the year 1920. Bull. N. Y. Bot. Gard. **10**: 307-322. September 10, 1921.

[Plant nomenclature: more suggestions] Jour Bot. **59**: 296, 297. October, 1921.

George Valentine Nash. Jour. N. Y. Bot. Gard. **22**: 145-147, *pl. 261 (portr.)*. "August" [October] 1921

Lathyrus latifolius. Everlasting pea. Addisonia **6**: 35, 36, *pl. 210*. October 24, 1921.

Dr. Pennell's new position. Jour. N. Y. Bot. Gard. **22**: 171, 172. "September" [December 8] 1921.

1922

Studies of West Indian plants.—X. Bull. Torrey Club **48**: 327-343. February 28, 1922. Also as Contr. N. Y. Bot. Gard. no. 236.

Botanical investigations in Porto Rico. Jour. N. Y. Bot. Gard. **23**. 49-59. "April" [June] 1922.

Report of the Secretary and Director-in-Chief for the year 1921. Bull. N. Y Bot. Gard **12**: 1-15. July 6, 1922.

Notes on the plants collected by Mr. Bucher on Pico Turquino, Cuba. (With Percy Wilson.) Jour. N. Y. Bot. Gard. **23**: 91-95. "July" [August] 1922.

Two new genera of Cactaceae. (With Joseph Nelson Rose.) Bull. Torrey Club **49**: 251, 252. August 31, 1922.

Polystachya minuta. Polystachya. Addisonia **7**: 19, *pl. 234*. September 25, 1922.

Xylophylla Epiphyllanthus. Hardhead. Addisonia **7**: 31, *pl. 240*. September 25, 1922.

The Cactaceae. Descriptions and illustrations of plants of the Cactus family. (With Joseph Nelson Rose.) Volume III. i-vii, 1-255, *f. 1-250; pl. 1-24*. October 12, 1922. Carnegie Institution of Washington, Publication no. 248, volume III.

Crotalaria retusa. Large yellow-rattlebox. Addisonia **7**: 47, *pl. 248*. December 4, 1922.

1923

Studies of West Indian plants.—XI. Bull. Torrey Club **50**: 35-56. February 7, 1923. Also as Contr. N. Y. Bot. Gard. no. 238.

Three new plants of the family Rubiaceae from Trinidad. (With Paul Carpenter Standley.) Jour. Wash. Acad. **13**: 105-107. March 19, 1923.

Cyperaceae. (Text, except Carex.) In: Abrams, Le Roy. An illustrated flora of the Pacific states **1**: 255-281. 1923.

Report of the Secretary and Director-in-Chief for the year 1902. Bull. N. Y. Bot. Gard. **12**: 103-115 June 7, 1923.

Botanical exploration of Porto Rico and the Virgin Islands. Jour. N. Y. Bot. Gard. **24**: 93-99. "May" [June] 1923.

Preface [to the Botany of Porto Rico and the Virgin Islands.] N. Y. Acad. Sci. Surv. Porto Rico **5**: 1-5. August 10, 1923.

Botany of Porto Rico and the Virgin Islands. Descriptive flora.—Spermatophyta (part). (With Percy Wilson.) N. Y. Acad. Sci. Surv. Porto Rico **5**: 7-158. August 10, 1923.

An unrecorded weed in Bermuda. Torreya **23**: 67, 68. August, 1923.

Bignonia radicans. Trumpet-creeper. Addisonia **8**: 39, 40. *pl. 276*. November 10, 1923.

Phlebotaenia Cowellii. Violet-tree. Addisonia **8**: 47, 48, *pl. 280*. November 10, 1923.

The Cactaceae. Descriptions and illustrations of plants of the Cactus family. (With Joseph Nelson Rose.) Volume IV. i-vii, 1-318, *f. 1-263; pl. 1-37*. December 24, 1923. Carnegie Institution of Washington, Publication no. 248, volume IV. "Pages 1-80, text only, were distributed under date of October 9, 1923."

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Francis Alexander Schilling. Jour. N. Y. Bot. Gard. **24**: 283, 284. "December, 1923" [January, 1924].

Botany of Porto Rico and the Virgin Islands. Descriptive flora.—Spermatophyta (continued). (With Percy Wilson.) N. Y. Acad. Sci. Surv. Porto Rico **5**: 159-316. January 10, 1924.

An extraordinary December. Jour. N. Y. Bot. Gard. **25**: 15, 16. January, 1924.

Plants of the Galapagos Islands. Jour. N. Y. Bot. Gard. **25**: 10, 11. "January" [February] 1924.

Hamelia axillaris. Yellow hamelia. Addisonia **8**. 51, *pl. 282*. February 5, 1924.

Lagetta Lagetto. Lace-bark tree. Addisonia **8**: 61, *pl. 287*. February 5, 1924.

Studies of West Indian plants.—XII. Bull. Torrey Club **51**: 1-12. February 8, 1924. Also as Contr. N. Y. Bot. Gard. no. 252.

La vegetación de cayo Icacos. Revista Agr. Puerto Rico **12**: 91-96. February, 1924.

The Scientific Survey of Porto Rico and the Virgin Islands. (Estudios cientificos sobre Puerto Rico y las Islas Virgenes.) Revista Agr. Puerto Rico **12**: 109, 110. February, 1924.

Las arenas de las planicies de la costa norte de Puerto Rico. Revista Agr. Puerto Rico **12**: 157, 158. March, 1924.

Porto Rico coffee slump. Tea & Coffee Trade Jour. **46**: 653, 654. May, 1924.

Report of the Secretary and Director-in-Chief for the year 1923. Bull. N. Y. Bot. Gard. **12**: 181-193 May 28, 1924.

Descriptive flora of Porto Rico and the Virgin Islands. Jour. N. Y. Bot. Gard. **25**: 129-135. May, 1924.

Botany of Porto Rico and the Virgin Islands. Descriptive flora.—Spermatophyta (continued). (With Percy Wilson.) N. Y. Acad. Sci. Surv. Porto Rico **5**: 317-474. June 16, 1924

A botanical expedition to extreme western Cuba. Jour. N. Y. Bot. Gard. **25**: 194, 195. July, 1924.

Summer work with irises. Jour. N. Y. Bot. Gard **25**: 196, 197. July, 1924.

Acer rubrum. Red maple. Addisonia **9**: 19, 20, *pl. 298*. July 30, 1924.

Botany of Porto Rico and the Virgin Islands. Descriptive flora.—Spermatophyta (continued). (With Percy Wilson.) N. Y. Acad. Sci. Surv. Porto Rico **5**: 475-626. November 8, 1924.

Asterandra grandifolia. Tobillo. Addisonia **9**: 41, *pl. 309*. November 15, 1924.

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Botany of Porto Rico and the Virgin Islands. Descriptive flora.—Spermatophyta (continued). (With Percy Wilson.) N. Y. Acad. Sci. Surv. Porto Rico **6**: 1-158. January 14, 1925.

The pinetum, the collection of evergreen trees. Jour. N. Y. Bot. Gard. **26**: 1-3. "January" [February] 1925.

Resignation of Doctor Murrill. Jour. N. Y. Bot. Gard. **26**: 13. "January" [February] 1925.

El plantel del servicio forestal de Puerto Rico. Revista Agr. Puerto Rico **14**: 83, 84. February, 1925.

Botany and horticulture of Porto Rico and the Virgin Islands. Jour. N. Y. Bot. Gard. **26**: 97-102. May, 1925.

The Charles Patrick Daly and Maria Lydig Daly Fund. Jour. N. Y. Bot. Gard. **26**: 161. July [25], 1925.

Report of the Secretary and Director-in-Chief for the year 1924. Bull. N. Y. Bot. Gard. **13**: 7-17. July 30, 1925.

Barleria Prionitis. Prionitis. Addisonia **10**: 17, *pl. 329*. August 6, 1925.

Urena lobata. Cadillo. Addisonia **10**: 19, *pl. 330*. August 6, 1925.

Erythrina Poeppigiana. Bucare. Addisonia **10**: 21, *pl. 331*. August 6, 1925.

Byrsonima Horneana. Mrs. Horne's byrsonima. Addisonia **10**: 23, *pl. 332*. August 6, 1925.

Tabebuia haemantha. Roble colorado. Addisonia **10**: 25, *pl. 333*. August 6, 1925.

Barbieria pinnata. Enredadera. Addisonia **10**: 27, *pl. 334*. August 6, 1925.

Chamaecrista mirabilis. Porto Rico partridge-pea. Addisonia **10**: 29, *pl. 335*. August 6, 1925.

Distictis lactiflora. Liana fragrante. Addisonia **10**: 31, *pl. 336*. August 6, 1925.

Botany of Porto Rico and the Virgin Islands. Descriptive flora.—Spermatophyta (continued). (With Percy Wilson.) N. Y. Acad. Sci. Surv. Porto Rico **6**: 159-316. August 31, 1925.

Third grant from income of the Charles Budd Robinson Memorial Fund. Jour. N. Y. Bot. Gard. **26**: 195, 196 September [30], 1925.

The American oaks. Jour. N. Y. Bot. Gard. **26**: 205. September [30], 1925. (Review.)

The tree-cactuses of the West Indies. (With Joseph Nelson Rose.) Jour. N. Y. Bot. Gard. **26**: 217-221. October, 1925. [Illust.]

1926

An attempt to aid the natural propagation of hemlocks. Jour. N. Y. Bot. Gard. **27**: 6-9. January [21], 1926.

Bequest of Helen C. Inslee. Jour. N. Y. Bot. Gard. **27**: 17, 18. January [21], 1926.

A dwarf plantation. Jour. N. Y. Bot. Gard. **27**: 25-29. February [10], 1926.

Tree-ferns in Porto Rico. Jour. N. Y. Bot. Gard. **27**: 88-90, *f. 4*. April [16], 1926.

Further botanical investigation in Porto Rico. Jour. N. Y. Bot. Gard. **27**: 97-102. May [27], 1926.

Report of the Secretary and Director-in-Chief for the year 1925. Bull. N. Y. Bot. Gard. **13**: 95-108. June 3, 1926.

Botany of Porto Rico and the Virgin Islands. Descriptive flora.—Spermatophyta, with appendix (concluded). (With Percy Wilson.) N. Y. Acad. Sci. Surv. Porto Rico **6**: 317-371. June 15, 1926.

Osmia borinquensis. Porto Rico osmia. Addisonia **11**: 9, *pl. 357*. June 30, 1926.

A portrait of Mrs. Daly. Jour. N. Y. Bot. Gard. **27**: 135. June [30], 1926.

Doctor William E. Wheelock. Jour. N. Y. Bot. Gard. **27**: 186. August [7], 1926.

The swamp cypresses. Jour. N. Y. Bot. Gard. **27**: 205-207. September [27], 1926.

Fourth grant from the income of the Charles Budd Robinson Fund. Jour. N. Y. Bot. Gard. **27**: 234. October [15], 1926.

Studies of West Indian plants.—XIII. Bull. Torrey Club **53**: 457-471. October 30, 1926. Also as Contr. N. Y. Bot. Gard. no. 284.

The American species of Stipa. Torreya **26**: 93-95. October 30, 1926. (Review.)

The David Lydig Fund. Jour. N. Y. Bot. Gard. **27**: 251, 252. "November" [December 4] 1926.

Teedia lucida. Shining teedia. Addisonia **11**: 37, *pl. 371*. December 9, 1926.

1927

Ipomoea quinquefolia. Small white morning-glory. Addisonia **11**: 63, *pl 384*. January 5, 1927.

The William R Sands Fund. Jour. N. Y Bot. Gard. **28**: 23. January [28], 1927.

The Henry Iden Fund. Jour. N. Y. Bot. Gard. **28**. 45, 46. "February" [March 3] 1927.

A portrait of Judge Charles P. Daly. Jour. N. Y. Bot. Gard. **28**: 46. "February" [March 3] 1927.

Studies of the mimosas. Jour. N Y. Bot Gard. **28**: 67, 68. March [16], 1927.

The Darius Ogden Mills Fund. Jour. N. Y Bot. Gard. **28**: 68, 69. March [16], 1927.

The Addison Brown Fund. Jour. N. Y. Bot. Gard. **28**· 89, 90. April [13], 1927.

Trees of Coamo Springs Hotel. 1-14. April, 1927. Los arboles de los Baños de Coamo. 1-15. April, 1927.

The Maria De Witt Jesup Fund. Jour. N. Y. Bot. Gard. **28**: 119, 120. May [17], 1927.

Report of the Secretary and Director-in-Chief for the year 1926. Bull. N. Y. Bot. Gard. **13**: 181-190. June 8, 1927.

Further botanical studies in Porto Rico. Jour. N. Y. Bot. Gard. **28**: 125-131, *f. 1, 2* June [10], 1927.

The Fanny Bridgham Fund. Jour. N. Y. Bot. Gard. **28**: 148, 149. June [10], 1927.

Sabinea punicea. Caracolillo. Addisonia **12**: 21, *pl. 395*. June 30, 1927.

Angelonia salicariaefolia. Angelon. Addisonia **12**: 23, 24, *pl. 396* June 30, 1927.

Planting of young hemlocks in the Hemlock Grove. (With Barrington Moore.) Jour. N. Y. Bot. Gard. **28**: 172, 173. July [9], 1927.

Bequest of Dr. George Newton Best. Jour. N. Y. Bot. Gard. **28**· 173, 174. July [9], 1927.

Opening address [at the International conference on flower and fruit sterility, August 12-14, 1926]. Mem. Hort. Soc. N. Y. **3**: 1. July, 1927.

The Francis Lynd Stetson Fund. Jour. N. Y. Bot. Gard. **28**: 200. August [5], 1927.

Stahlia monosperma. Cobana negra. Addisonia **12**: 33, 34, *pl. 401*. October 28, 1927.

Exogonium arenarium. Cambustera. Addisonia **12**: 35, *pl. 402*. October 28, 1927.

Niopa peregrina. Cojobana. (With Joseph Nelson Rose.) Addisonia **12**: 37, *pl. 403*. October 28, 1927.

Erythrina Corallodendrum. Coral tree. Addisonia **12**: 39, *pl. 404*. October 28, 1927.

Chamaefistula antillana. Hediondilla. (With Joseph Nelson Rose.) Addisonia **12**: 41, *pl. 405*. October 28, 1927.

Canavali maritima. Bay bean. Addisonia **12**: 45, *pl. 407*. October 28, 1927.

The Iris Garden entrance and boundary fence adjoining. Jour. N. Y. Bot. Gard. **28**: 266, 267. November [10], 1927.

Charles Frederick Rand. Jour. N. Y. Bot. Gard. **28**: 302, 303. December [27], 1927.

1928

Doctor W. Gilman Thompson. Jour. N. Y. Bot. Gard **29**: 13, 14. January [28], 1928.

Mimosaceae. (With Joseph Nelson Rose.) N. Am. Flora **23**: 1-76. February 11, 1928.

Resolutions relative to the services of Doctor Frederic S. Lee to the New York Botanical Garden. Jour. N. Y. Bot. Gard **29**: 42, 43. February [16], 1928.

Forestry and agriculture in Porto Rico. Jour. N. Y. Bot. Gard. **29**: 101-104. May [12], 1928.

Report of the Secretary and Director-in-Chief for the year 1927. Bull. N. Y. Bot. Gard. **13**: 257-260. May 31, 1928.

The John Innes Kane Fund. Jour. N. Y. Bot. Gard. **29**: 210, 211. August [16], 1928.

Mimosaceae (continuatio). (With Joseph Nelson Rose.) N. Am. Flora **23**: 77-136. September 25, 1928.

Bequest of the Burgess collection of asters. Jour. N. Y. Bot. Gard. **29**: 267, 268. October [20], 1928. Reprinted in Torreya **28**: 100, 101. November 16, 1928.

The herbarium of Eugene A. Rau. Jour. N. Y. Bot. Gard. **29**: 284, 285. November [30]. 1928.

Mimosaceae (conclusio). (With Joseph Nelson Rose.) N. Am. Flora **23**: 137-194. December 20, 1928.

Ipomoea polyanthes. Yellow morning-glory. Addisonia **13**: 49, *pl. 441*. December 31, 1928.

Jussiaea angustifolia. Yerba de clavo. Addisonia **13**: 51, *pl. 442*. December 31, 1928.

Tetrazygia elaeagnoides. Cenizo. Addisonia **13**: 53, *pl. 443*. December 31, 1928.

Chamaecrista Swartzii. Tamarindillo. Addisonia **13**: 55, *pl. 444*. December 31, 1928.

Columnea Tulae. Tibey parasitico. Addisonia **13**: 57, *pl. 445*. December 31, 1928.

Rubus rosaefolius. Mountain raspberry. Addisonia **13**: 59, *pl. 446*. December 31, 1928.

Volkameria aculeata. Prickly myrtle. Addisonia **13**: 61, *pl. 447*. December 31, 1928.

Pentarhaphia albiflora. Porto Rico pentarhaphia. Addisonia **13**: 63, *pl. 448.* December 31, 1928.

1929

[Botanical nomenclature.] Proc. Internat. Congr. Pl. Sci. Ithaca. 1569, 1570. [April] 1929.

Further studies in Porto Rico. Jour. N. Y. Bot. Gard **30**: 101-105. May [14], 1929

Report of the Secretary and Director-in-Chief for the year 1928. Bull. N. Y. Bot. Gard. **13**: 351-354. May 31, 1929

The shrub yellow-root. Jour. N. Y. Bot Gard. **30**. 265-269, *f. 1.* November [14], 1929.

Pseudocopaiva Britton & Wilson, gen. nov. (With Percy Wilson.) Trop. Woods **20**: 28. December 1, 1929.

1930

The planting of a Stahlia on Arbor Day in Porto Rico. Jour. N. Y. Bot. Gard. **31**: 45-47. "February" [March 13] 1930.

The native Cactaceae of Porto Rico and the Virgin Islands. Jour. Cact. Succ. Soc. Am. **1**: 226-231. June, 1930.

Scientific Survey of Porto Rico and the Virgin Islands. Jour. N. Y. Bot. Gard. **31**: 161-167. July [21], 1930.

Texas cacti. Jour. Cact. Succ. Soc. Am. **2**: 285, 286. August, 1930. (Review.)

Scientific survey of Porto Rico and the Virgin Islands. Rev. Obr. Publ. Puerto Rico **7**: 242-245, *map.* September, 1930.

The cacti of Cuzco. Jour. Cact. Succ. Soc. Am. **2**: 308. September, 1930. (Review.)

Foreword. In: Houghton, Arthur Duvernoix. The cactus book. vii, viii. October, 1930.

Sabinea florida. Watapama. Addisonia **15**: 33, 34, *pl. 497.* October 25, 1930.

Corynella paucifolia. Retama. Addisonia **15**: 35, 36, *pl. 498.* October 25, 1930.

Jacquemontia subsalina. Subsaline jacquemontia. Addisonia **15**: 37, *pl. 499.* October 25, 1930.

Proustia Krugiana. Consul Krug's proustia. Addisonia **15**: 39, *pl. 500.* October 25, 1930.

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William Morton Wheeler

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OF

WILLIAM MORTON WHEELER

1865–1937

BY

GEORGE HOWARD PARKER

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WILLIAM MORTON WHEELER

1865–1937

BY GEORGE HOWARD PARKER

William Morton Wheeler, son of Julius Morton Wheeler and Caroline Georgiana (Anderson) Wheeler, was born in Milwaukee, Wisconsin, March 19, 1865. Of his very early childhood little or nothing is recorded, but of his school days and early life Wheeler has left a sketch from his own pen that carries with it all the freshness and energy of youth. This sketch is contained in an article published in "Natural History" (1927) and entitled "Carl Akeley's Early Work and Environment." Akeley became one of Wheeler's early and most intimate friends and Wheeler's appreciation of him contains so much that is autobiographical that it would be difficult to do better in recording Wheeler's own youthful experiences than to cite directly from this source.

Wheeler wrote: "I was born in 1865 in Milwaukee and lived there till I was nearly nineteen. The cerevisiacal fame which that city enjoyed in those preprohibition days unfortunately quite eclipsed the fame of its temperate and highly intellectual German population and excellent school system.

"Owing to my persistently bad behavior soon after I entered the public school my father transferred me to a German academy founded by Peter Engelmann, an able pedagogue who had immigrated to the Middle West in 1848. The school had a deserved reputation for extreme severity of discipline. To have annoyed one of the burly Ph.D.'s, who acted as my instructors, as I had annoyed the demure little schoolmarms in the ward school, would probably have meant maiming for life at his hands or flaying alive by the huge Jewish director, Dr. Isidore Keller, 'curled and oiled like an Assyrian bull'.

"After completing the courses in the academy, I attended a German normal school which somehow had come to be appended to the institution. A few weeks before my father's death in January, 1884, an incident occurred which was to influence my whole subsequent life and indirectly Carl Akeley's. Prof. H. A. Ward, proprietor of Ward's Natural Science Establishment in Rochester, New York, which was not so much a museum as a museum factory, learned that there was to be an exposition in Milwaukee in the fall of 1883 and that the local German

academy, which I had attended, possessed a small museum. He decided, therefore, to bring a collection of stuffed and skeletonized mammals, birds, and reptiles, and an attractive series of marine invertebrates to the exposition, and to persuade the city fathers to purchase the lot, combine it with the academy's collection, and thus lay the foundation for a free municipal museum of natural history. I had haunted the old academy museum since childhood and knew every specimen in it. Indeed, Dr. H. Dorner, my instructor in natural science, had often permitted me to act as his assistant. Of course, I was on hand when Professor Ward's boxes arrived, and I still remember the delightful thrill with which I gazed on the entrancing specimens that seemed to have come from some other planet. I at once volunteered to spend my nights in helping Professor Ward unpack and install the specimens, and I worked as only an enthusiastic youth can work. He seems to have been duly impressed by my industry, because he offered me a job in his establishment. I was quite carried away with the prospect of passing my days among the wonderful beasts in Rochester. Not the least of Professor Ward's attainments were his uncanny insight into human nature and his grim business and scientific acumen. He offered me the princely salary of nine dollars a week, six of which were to be deducted for board and lodging in his own house.

"I entered Ward's Establishment February 7, 1884. My duties consisted in identifying, with the aid of a fair library, and listing birds and mammals. Later I was made a foreman and devoted most of my time to identifying and arranging the collections of shells, echinoderms, and sponges, and preparing catalogues and price lists of them for publication. Such is the present state of conchology that my shell-catalogue is still used by collectors. At this time Akeley entered the establishment as a budding taxidermist, and for once Professor Ward's estimate of human nature seems to have been at fault, for as Akeley informs us in *In Brightest Africa* he was given a salary of $3.50 a week, without board and lodging. He attached himself to William Critchley, a young and enthusiastic artisan, with the voice and physique of an Italian opera tenor, who had attained the highest proficiency in the taxidermic methods of the time, but did not seem to give promise of advancing the art. In the course of a year Akeley had more than mastered all that Critchley could teach him, and was longing for wider opportunities than could be offered by an establishment, which, after all, was neither an art school nor a scientific laboratory, but a business venture. But even so, there is reason to believe that its stand-

ards of workmanship were higher than in any of the museums that had grown up in various parts of the country.

"The relations between Akeley and myself soon ripened into a warm friendship. We were nearly of the same physical age, but I was the younger and more unsettled mentally, for he had been reared by sturdy parents on a quiet farm and I had been brought up in a bustling city with a superheated atmosphere of German Kultur. He was very strong and healthy, had an inexhaustible capacity for work, a great fund of quiet humor, and a thoroughly manly disposition. He seemed to have been born with unusual taste and discrimination and an intuition which could dispense with mere book-learning. Of all the men I have known—and my profession has brought me in contact with a great many—he seems to me to have had the greatest range of innate ability. Although he later became an unusual sculptor, inventor, and explorer, he would probably have been equally successful in any other career.

"In the course of time our relations settled into those of affectionate older and younger brothers. I cannot recall that we were ever even on the verge of a quarrel, and this must have been due to Akeley's self-restraint and sympathetic tolerance, because I was often irritable and unwell in those days. Owing to the fact that we did not work in the same building, our companionship was largely limited to evenings and Sundays. As I read the diaries of 1884 and 1885 I marvel at the multiplicity of our youthful interests and occupations. I cite a few passages to illustrate how we spent some of our spare hours.

'Monday, Jan. 6, 1885.* Worked on the glossary for the shell-catalogue all day. In the evening went with Carl to hear Bob Ingersoll in his lecture "Which Way?" We were much pleased with him and his wit. The lecture cleared from my mind a host of prejudices against this man who is after all a *real he man.* Weather cold.'

'Sunday, Feb. 15, 1885. Rose late. Took a walk with Carl and then went to church (Unitarian) with him to hear Doctor Mann give a magnificent sermon on the text "Out of Egypt will I call my son." Worked on algebra and read Virgil after dinner. Then walked down West Ave. with Fritz Mueller (a former schoolmate whom I was coaching in Latin for entrance to Johns Hopkins. He was the living image of the famous physiologist Johannes Mueller and probably belonged to the same family). Tired on my return. Fritz read to me Jean Paul Friedrich Richter's "Kampaner Thal."'

* In this and other starred quotations the day of the week and the day of the month do not coincide for the year 1885; as approximations they are sufficient.

'Thursday, Feb. 26, 1885. Worked on the shell-catalogue more diligently than on previous days, but am still low-spirited. In the evening read the conclusion of the Aeneid and some of Zeller's "Deutches Reich" with Louis Akeley (Carl's brother who was attending the University of Rochester and whom I was coaching in German). To bed at quarter of twelve.'

'Monday, March 23, 1885. Worked all day on the foetal Marsupials: kangeroos, koalas, opossums, etc. Labelled all the foetuses and pouches. In the evening walked with Fritz and on returning read with him about 100 lines of the third book of the Aeneid. The evening ended with an acrimonious dispute and I went to bed in high dudgeon.'

'Thursday, March 24, 1885.* Worked all day in Prevotel's shop, changing and labelling the alcoholic fishes. In the evening attended the meeting of the Geological Section of the Rochester Academy of Sciences. Mr. Preston read to us about a quarter of Geikie's "Primer of Geology." After the meeting walked with Mr. Shelley Crump (an amateur conchologist and prosperous grocer of Pittsford, New York, to whom I had become greatly attached). To bed at eleven.'

And this is an account of a week-end with Mr. Crump:

'Sunday, May 23, 1885.* From 10 to 12 worked with Professor Ward in the shell-house, labelling Echini—the last time I saw him (for many years). In the afternoon Mr. Crump and his friend, Doctor Dunning, called on me. I walked with them to Brighton and thence took the train to Pittsford. We read together some recent papers on Pasteur by Tyndall and others and then walked along the Erie Canal bank where I collected two species of Valvata.'

'Monday, May 24, 1885.* Rose late. Read some of Burrough's 'Wake Robin' before breakfast. Then conversed with Dr. Dunning on Shakespeare's 'Sonnets' (Dr. D. was blind and with the aid of his wife was preparing a volume on the sonnets). At 9.20 took the train for Rochester and went to work in the shell-house finishing the family Nassidae and part of the Volutidae.'

'Tuesday, June 23, 1885. In the morning read Bluntschli with Louis Akeley. In the afternoon went with Carl, Will Critchley, and Mr. Crump to see the tobacconist Kimball's beautiful collection of orchids. Succeeded in making a Catasetum discharge its pollinia! In the evening read Bluntschli again after having seen Mr. Crump off on the West Shore train. Returned much fatigued. My eyes begin to pain me.'

"Of active, industrious young men there seem to be two types. One of them accepts a given environment and is not only

satisfied with its routine and constantly recurring human contacts but prefers it to any change. These young men are apt to marry early and to become the conservative and contented *fond* of our society. Those of the other type, probably endowed with a more unstable if not more vivid imagination and with a peculiar defence reaction, or subconscious dread of being owned by people and things, soon exhaust the possibilities of their medium, like fungi that burn out their substratum, and become dissatisfied and restless till they can implant themselves in fresh conditions of growth. Akeley and I were of this latter type, and by the spring of 1885 had decided to leave the establishment at the earliest opportunity. I departed June 29 and returned to Milwaukee.

"Soon after my return to Milwaukee my old friend, Dr. George W. Peckham, who had long been making important contributions to arachnology and was beginning his well-known studies on the behavior of the solitary and social wasps, persuaded me to take a position as teacher of German and physiology in the high school of which he was principal. Peckham was a very learned and charming man, deeply steeped in the evolutionary literature of the time and keenly alive to the possibilities of the new morphology that had been inaugurated by Huxley in England and a host of remarkable investigators in the laboratories of the German universities. Every year he most conscientiously read, as a devout priest might read his breviary, Darwin's *Origin* and *Animals and Plants under Domestication*. We became very intimate, and I find from my diaries that for some years I regularly spent my Sunday mornings in his house drawing the palpi and epigyna of spiders to illustrate the papers which he wrote in collaboration with his equally gifted and charming wife. I was privileged to collaborate with them in one paper (on the Lyssomanae) and to help them during the summers in their field work on the wasps at Pine Lake, Wisconsin. Under Peckham's management the biological work of the Milwaukee high school was carried far beyond that of any similar institution in the country. There were classes in embryology with Foster as a text. We possessed a Jung microtome and the paraphernalia for staining sections and demonstrating the development of the chick, and, of course, the classes in physiology were required to master Huxley and Martin. While at Ward's I had purchased Carnoy's *Biologie Cellulaire* and had imbibed from it an intense but rather ineffectual interest in cytology. Then most fortunately, Mr. E. P. Allis established his 'Lake Laboratory' in his residence near the high school and appointed Prof. C. O. Whitman as its director and Dr. William Patten, Dr. Howard Ayres, and Mr.

A. C. Eycleshymer as assistants. These gentlemen were, of course, actively spreading the gospel of the new morphology. Doctor Patten, only four years my senior and fresh from Leuckart's laboratory in Leipzig, taught me the latest embryological technique and suggested that I take up the embryology of *Blatta* and other insects. I find that I devoted nearly all my spare time to this work till 1890.

"In the meantime the Milwaukee Public Museum had been established according to the plan suggested by Professor Ward, and I saw an opening for Akeley as its taxidermist. I persuaded him to come to Milwaukee and live with me. He arrived November 8, 1886, and although he was not officially appointed to the institution till November 20, 1888, he was given a certain amount of its work. We converted a barn on my mother's place into a shop and here he worked at least during the evenings for several years. I was made custodian of the Museum, September 19, 1887, and held the position till August 29, 1890. By that time my association with Peckham, Whitman, and Patten had converted me into a hard-boiled morphologist, and I was induced by Whitman to accept a fellowship at Clark University, where he had become professor of zoology a year earlier. Till October 1, 1890, when I left Milwaukee for good, Akeley and I had spent so many happy hours together that the parting was painful. After leaving the high school I had fitted up a laboratory in the house and when my eyes grew weary with the microscope I repaired to his shop and read to him while he worked or more rarely he read to me. My diary mentions the volumes we read and I wonder at Akeley's patience and apparent pleasure in listening to Bryce's *American Commonwealth,* translations of Aeschylus, Max Nordau, and similar highbrow stuff. I patiently read a whole small library for at that time I had serious conscientious objections to beginning a book without reading its every word. Perhaps Akeley really heard only occasional important fragments and had found that he could carry on his own trains of inventive thought better when we were together and I was making a continual but not too disturbing noise."

Such is the glimpse that we can gain into Wheeler's early life as recorded by himself. During this period he had graduated from the German-American Normal College (1884), had worked for the greater part of a year as an assistant in Ward's Natural Science Establishment (1884-1885), had taught German and physiology in the Milwaukee High School (1885-1887), and had served as Custodian of the newly established Milwaukee Public Museum (1887-1890), an amazing degree of activity

for one just turned twenty-five years of age. Looked on as a preparation for future work this body of training and experience could scarcely have been excelled. Devoid of the restraints of academic surroundings and free to expand by normal means, Wheeler's youthful growth was one of unexampled progress. Well grounded in languages both ancient and modern, conversant with the historical past, and filled with enthusiasm for biology and its future, he was ready for his life-long work in productive scholarship. Exceptional as this outlook was, it is remarkable how naturally and simply it was attained. Free of the cumbersome conventions of an educational system, Wheeler moved directly and without embarrassment to the end in view.

The formal beginnings of scholarly output from any research worker are as a rule shown in his publications. In this Wheeler made an early start. Probably his first published article was the catalogue of mollusks and brachiopods already referred to in his diary and prepared for Ward's Natural Science Establishment. This catalogue, which was by no means a mere price-list of the materials available at Ward's, was used for many years by conchologists, both amateur and professional, in the classification and arrangement of their specimens. It was at once trustworthy, compact, and inexpensive. It was put forth anonymously and without date, like a picture by an Italian primitive, but those who used it knew its author. On his return from Rochester to Milwaukee, Wheeler prepared a list of the trees of his native city (1885) and the next year he published his first entomological paper, an account of the beetles from the lake beaches of Milwaukee County. Thus began that incomparable series of scientific publications that reached without interruption from this early period to the time of his death.

By a strange but fortunate coincidence, Milwaukee in the later years of Wheeler's residence there became a center of unusual zoological activity. The director of the Lake Laboratory, Dr. Whitman, and two of his assistants had recently returned from study in the European zoological centers and were filled with enthusiasm for the new morphology, its fascinating problems, and how to attack them. In this company, Wheeler found himself a welcome guest and soon became, to use his own ex-

pression, "a hard-boiled morphologist." At about this time, 1887, Whitman, with the cooperation of E. P. Allis, Jr., launched the new *Journal of Morphology* and the next year, 1888, he undertook the establishment of the Marine Biological Laboratory at Woods Hole, Massachusetts. To the small circle of workers in Milwaukee these days must have been days full of feverish excitement. Wheeler once related that while he and Patten were walking from the Lake Laboratory, Patten was suddenly taken with an idea about the ancestry of the vertebrates and, as was characteristic of him, elaborated the whole matter on the spot and at great length. This idea, that vertebrates were derived from arachnid ancestors, subsequently occupied Patten during the greater part of his life, but in Milwaukee it struck him all in a moment. It was Patten also who instigated in Wheeler the desire to study insect embryology and suggested to him that he take up the investigation of the development of the common cockroach. This subject occupied much of Wheeler's spare time in his later years at Milwaukee where its investigation was carried on by him in part at the Milwaukee High School and in part at the Lake Laboratory. In 1889 it appeared under the title of "The Embryology of Blatta germanica and Doryphora decemlineata" in the third volume of the *Journal of Morphology*. This study was followed in 1893, after Wheeler had gone to Clark University, by his "Contribution to Insect Embryology," also published in Whitman's new journal. These two papers have long been recognized as classics in their fields of research. The first, done in Milwaukee, is a tribute to the intense zoological activities of the place and particularly of the Lake Laboratory. Here researches in other directions and by other workers were progressing with prodigious strides, and into this whirl of scientific activity Wheeler threw himself without reserve.

But the Lake Laboratory was not to maintain itself long. It soon lost its first director, Dr. Whitman, after which it steadily declined. Other institutions were arising. Clark University had been founded for research in Worcester, Massachusetts, and distinguished scholars in many fields were being called to it. The eminent psychologist, Dr. G. Stanley Hall, was its new

president and through him Dr. Whitman was invited to be head of its department of biology. Whitman accepted the offer and was followed by Wheeler who in 1890 became Fellow and Assistant in Morphology at Clark. Here new associations were to be made, and Wheeler found himself on terms of growing intimacy with Dr. Sho Watase, the promising Japanese zoologist direct from the biological laboratories of Johns Hopkins University. Dr. Jacques Loeb, the brilliant young general-physiologist on a brief visit to Clark also made Wheeler's acquaintance. Both these men particularly in consequence of their later association with Wheeler in Chicago became his life-long friends.

During his sojourn at Clark, Wheeler continued his work on insects and published in this period some ten papers almost all of which were entomological in substance. In 1892 he presented himself as a candidate for the degree of Ph.D. on the basis of his work on insect embryology and Clark University granted him that degree.

But the situation at Clark was not a happy one. The members of its faculty were newly brought together and, never having been associated before, their relations were not without friction. As a research university, Clark did not especially encourage the coming of a body of students and consequently the lack of flow through its gates of the young life so essential to the welfare of all such institutions made itself felt, especially among certain of the older men. An atmosphere of discontent arose and openings in other universities were sought by those who only one or two years before had looked upon Clark as a scholar's Utopia. Dr. Whitman received a call from the newly opened University of Chicago. This he accepted and carried with him to this new academic center Dr. Watase and Dr. Wheeler. Thus Wheeler in 1892 became Instructor in Embryology at Chicago under Dr. Whitman. This post he held till 1897 when he was advanced to Assistant Professor in his chosen subject.

As a preparation for his new duties in Chicago, Wheeler spent the academic year of 1893-1894 in Europe. He first went to the Zoological Institute at the University of Würzburg whose new director, Professor T. Boveri, had just succeeded the late

Professor C. Semper, the founder of the new Institute. Here Wheeler made first-hand acquaintance with student life in a German university. Part of the winter of 1893-1894 he spent at the Naples Zoological Station whose genial director, Dr. Anton Dohrn, did much to advance his interest in marine zoology. At the Naples Station, Wheeler occupied the table supported by the Smithsonian Institution. Though an inland man by both birth and training Wheeler's first acquaintance with marine life was not at Naples, for he had already spent, while in America, the summers of 1891 and 1892 at the Marine Biological Laboratory at Woods Hole. But the fauna at Naples was a great novelty to him and an unending stimulus to research. Here he began his studies on the sex life of Myzostoma, a subject which he carried with him to the Institut Zoologique at Liége, Belgium, where he worked in the laboratory of Professor E. Van Beneden. Subsequently his monograph on Myzostoma was published by Van Beneden in the *Archives de Biologie* (1897).

On his return to America in 1894 Wheeler settled down in Chicago to five years of active university work as a teacher of embryology. Of the score or more papers published by him during this period about half of them have to do with insects showing the predominantly entomological trend of his interest, a trend that dated back to 1885 when in Milwaukee he met Dr. and Mrs. George W. Peckham. These two ardent and accomplished entomologists fostered, as the extracts from Wheeler's diary show, his growing interests in the insect world.

In Chicago, Wheeler met, and on June 28, 1898, married Miss Dora Bay Emerson of Rockford, Illinois, a woman of great personal charm and delightful presence, who in the years that followed made his household a hospitable center for friends and for distinguished visitors from all quarters of the globe.

Wheeler's scientific interests though strongly entomological were never limited to this field. The fact that in Chicago he taught embryology for over five years is sufficient evidence of this. It is therefore not surprising when in 1899 he was offered the Professorship in Zoology with its wider outlook at the University of Texas, Austin, Texas, he should resign his position in Chicago and move to this southern institution. Here he remained

for about four years in what might be called an almost pioneer academic atmosphere, for the University of Texas combined at once persons of great refinement as well as those of a more rugged temperament. His publications over this period number about two score and are remarkable for the fact that almost three-fourths of them deal with ants, the group of insects which during the remainder of his days were to claim his chief attention. His other publications show an increasing breadth of scientific interest, for beside reviews in such diverse directions as Korschelt and Heider's "Textbook of Embryology" and Calkin's "Protozoa" he has much to say on the social life of ants, their mixed colonies, myrmecophiles, and the never-ending problem of organic evolution.

During this period students in his chosen field began to resort to him. C. T. Brues and A. L. Melander, both now well known entomologists, sought to study under Wheeler in Chicago, but having found him removed to the University of Texas, they made their way to Austin and spent several years there in his laboratory. Thus began an influx of younger, capable men who as pupils and scientific associates sought him out for longer or shorter periods of study and research under his guidance. During Wheeler's stay in Texas his two children were born, not, however, in Austin, but in Rockford, Illinois, the home town of his wife.

Rather overfed with the duties of teaching and of laboratory management, Wheeler was induced in 1903 to resign his position as Professor of Zoology in Texas and to accept the Curatorship of Invertebrate Zoology in the American Museum of Natural History in New York City. Here it fell to him to organize and arrange the Hall of Invertebrate Life and this beautiful exhibit with its remarkable display of specimens and its many truly wonderful glass models stands as a token of Wheeler's endless industry and good management. Behind the scenes he was occupied with work on the insects and, as his four score publications from this period show, his attention was devoted almost exclusively to the ants. His work on these insects was in no sense restricted, for he was active not only in the description of new species and in their classification but in their structure, func-

tions, distribution, habits and above all in their social relations and ecology. At no time during his earlier life had Wheeler so concentrated his activities on a special group of related problems as he did during his five years as Curator at the American Museum and at no time before had the results of his work been more brilliant and permanently enduring. The most conspicuous product of this period was the volume he contributed to the Columbia University Biological Series entitled "Ants: Their Structure, Development and Behavior." On the pages of this book are epitomized the intense work of a decade by one whose genius was at its full height.

But once a teacher always a teacher, and after five years of museum work, Wheeler felt the call of the lecture table, the laboratory, and the daily contact with aspiring young workers all of which together form an atmosphere, the nearest approach to a scientific scholar's ideal. Consequently when a call came to him to become Professor of Economic Entomology at the Bussey Institution of Harvard University he accepted it without reluctance and entered a new academic environment in which he was to remain longest of all. Here he worked almost thirty years, for in one capacity or another he was intimately associated with Harvard University from 1908 till his death in 1937. This final period in Wheeler's career must be looked upon as the one in which the great promise of his early days achieved complete realization and his genius ripened to full maturity. Over about two thirds of this period (1908-1926) he was Professor of Economic Entomology, a title which indicated the general trend of the Bussey Institution, but this title he preferred to change, and from 1926 to 1934 he served under the more general and certainly the more appropriate designation of Professor of Entomology. In 1934 he was made Professor of Entomology Emeritus. From 1915 to 1929 he was Dean of the Faculty of the Bussey Institution and from 1929 to 1937 he was Associate Curator of Insects in the Museum of Comparative Zoology. During the whole of the period of his association with Harvard University in recognition of his services at the American Museum of Natural History he was a Research Associate of that Museum.

Wheeler's entrance into the Bussey Institution came directly

after the reorganization of that body and he found himself associated there with a growing group of research workers in biology. At that time the Bussey Institution was one of the Graduate Schools of Applied Science under the deanship of Professor Wallace C. Sabine. On the dissolution of this body in 1914, the Bussey acquired a faculty of its own of which Wheeler was made dean (1915). Meanwhile Dr. W. E. Castle in animal genetics and Dr. E. M. East in plant genetics had joined this group and as an assistant to Wheeler had been added Mr. C. T. Brues, Instructor in Economic Entomology.

Following the establishment of the Bussey Faculty a number of other biologists joined its ranks and with its growth in advanced students the Bussey quickly became under Wheeler's leadership an institution for biological research, known the world over. As an administrative officer Wheeler was not always a complacent one for the university official to deal with. He was strenuously insistent that the institution of which he had charge should be properly manned and sufficiently supported and his insistence often brought him into conflict with those whose duty it was to provide the means to these ends. Never in any sense self-seeking, Wheeler nevertheless could on occasion assume a rigorously militant attitude when the general welfare of the Bussey was at stake and much of its remarkable growth at his hands depended upon the ability of its Dean to obtain resources from those who to him seemed to have but a niggardly conception of the functions of the Institution.

Wheeler's publications during this period numbered nearly three hundred. They were predominantly entomological and chiefly concerned with ants though they frequently dealt with these creatures in their most general phases. Many of his contributions had to do with the social life of ants and of other insects much of which was summarized, often with a delicately ironical turn, in his volumes "Social Life among the Insects" (1923), "Les Sociétés d'Insectes" (1926), "Foibles of Insects and Men" (1928), and "The Social Insects, their Origin and Evolution" (1928).

His interest in the philosophy of biology came to the surface in his vigorous espousal of Alexander's theory of emergent

evolution as shown in his article in Science "Emergent Evolution and the Social" (1926), and in his two booklets "Emergent Evolution and the Social" (1927) and "Emergent Evolution and the Development of Societies" (1928) in both of which he pointed out that any animal society was as much a soil for emergent growth as was the single creature. To him Hobbes' conception of society as an organism was a self-evident fact of nature. His historical feeling for his subject appeared in his discovery and translation of a lost manuscript by Réaumur, "The Natural History of Ants" (1926), and in the editing and publication with his colleague Dr. Thomas Barbour of "The Lamarck Manuscripts at Harvard" (1933). His essays of this period include such choice efforts as "The Termitodoxa, or Biology and Society" (1920), in which Wee-Wee, the Neotenic King of the 8,429th Dynasty of the Bellicose Termites discourses on the advantages of the white-ants' social life as compared with that of man, and "The Dry-Rot of Our Academic Biology" (1923) in which with cutting humor the "flubdub" of the academic biological world is laid bare. These are but a few of the choice fruits from the last of Wheeler's harvests.

When Wheeler in 1908 came to Harvard he took up residence in Jamaica Plain not far from the Bussey Building. As the Bussey was located in Forest Hills some eight miles from Cambridge his Harvard colleagues in natural history, mostly resident in Cambridge, saw relatively little of him. When the new Biological Laboratories were opened in Cambridge in September, 1931, in close proximity to the Museum of Comparative Zoology, it was decided to transfer the members of the Bussey Institution to this new location and provision was made for them in the new building. Wheeler with others came to the new situation and there began a life of much greater intimacy with the Cambridge biologists. Meanwhile in 1924 he had changed his residence from Jamaica Plain to Boston and thus came to live much nearer to the Cambridge centers. In his new Harvard surroundings he settled down with great complacency having two private laboratories, one in the Museum of Comparative Zoology among the insect collections and the other in the Biological Laboratories. That he spent more time in the latter than in the former resulted

from his habit of smoking while at work. Smoking because of fire risk was prohibited in the Museum, but was allowed in the Biological Laboratories.

Wheeler always arrived early in Cambridge for his day there, being usually driven in a car from Boston by his daughter. By nine o'clock he was to be found, as a rule, at his laboratory table in the biological building. Here he commonly worked till about noon, when he repaired to the Museum, where in the quarters of its Director, he took lunch. This mid-day rendezvous called by its frequenters "the eateria" was a center to which were invited many of the biological notables temporarily in Cambridge. It was therefore an interesting and stimulating gathering to which Wheeler added much and in which he took great delight. In the afternoon he usually worked either in his quarters in the Museum or in those in the Biological Laboratories. In the late afternoon he was driven back to his home where, if there were no social engagements, he was to be found in his study amidst books and manuscripts.

It was after a day much as that just described that he died suddenly of heart failure in Cambridge. He had dined at home and then for some unknown reason had been led to return to Cambridge, probably to make good some omission of the day. He could have stayed in Cambridge only a short time, for his death took place on the Boston-bound platform at the Harvard Subway Station early in the evening. This was on Patriots' Day, the nineteenth of April, 1937. He was survived by all his immediate family, his wife, Mrs. Dora Emerson Wheeler; his son, Dr. Ralph Emerson Wheeler; and his daughter, Miss Adeline Wheeler.

Wheeler was quietly fond of those he chose for his daily companions and he had in the best sense a warm heart for those nearest to him. Like his beloved ants, he was essentially social. In Boston he was often to be seen at the meetings of the Thursday Club and the dinners of the Academy Round Table and he was the center of a small group of older men who met informally at luncheon week by week in reminiscence of their European student days and early life. On all such occasions he was a

charming and delightful companion full of wit and rich in anecdote.

He was elected a member of the National Academy of Sciences in 1912 and attended its meetings with much regularity. He also held membership in many other scientific societies such as the American Philosophical Society, the American Academy of Arts and Sciences, the American Association for the Advancement of Science, the American Zoological Society of which he was a past president, the American Society of Naturalists, the American Ecological Society, of which he was at one time vice-president, the Philadelphia Academy of Natural Sciences, the Boston Society of Natural History, vice-president at the time of his death, the Washington Academy of Science, the New York Academy of Science, and, among foreign societies, the Zoological Societies of France and of Belgium, and the Entomological Societies of London, of France, and of Belgium. He was the recipient of the Elliot Medal from the National Academy of Sciences (1922) and of the Leidy Medal from the Philadelphia Academy of Natural Sciences (1931). In 1925 he served as Harvard Exchange Professor with France. He held honorary degrees from a number of universities: Doctor of Science from the University of Chicago (1916), from Harvard University (1930), and from Columbia University (1933) and doctor of Laws from the University of California (1933). In 1934 the French Republic made him an Officer in the Legion of Honor. Wheeler took a real pleasure in the distinctions bestowed upon him, but these honors never disturbed his unassuming demeanor and native modesty. The presentation of the Elliot Medal called from him an interesting account connected with his early friendship with Carl Akeley. It shows at once Wheeler's unbounded sense of humor even when the occasion was the reception of a very high honor, the Elliot Medal. It cannot be narrated better than in his own words:

"In 1894, soon after returning to the University of Chicago where I was then instructor in embryology with Professor Whitman, I learned that Akeley was at the Field Museum. I naturally looked forward to a renewal of our old intimacy but was informed that this was impossible. It seems that Professor Elliot, whom I had never met, disliked the zoological department

of the University, probably because of its strong morphological bias and the outspoken contempt of a few of its members for taxonomy, and I was naturally included as a *persona ingrata*. Moreover, he realized that he had captured a prize in Carl Akeley and was afraid that the secrets of his technique might leak out and be appropriated by some other museum. He therefore forbade any visits and kept Akeley closely confined, and as he worked every day and far into every night, I was able to see him only once or twice during all the years I was still to remain in Chicago. Professor Elliot's procedure was not devoid of humor, because I was, of course, perfectly familiar with Akeley's methods and could have made no use of them even had I wished to do so. Many years later fate brought an ironical atonement when the National Academy of Sciences conferred on me a medal which had been established by this same Professor Elliot!"

Wheeler was an omnivorous reader and he consumed volumes at a prodigious rate. His early and thorough training in languages received under the rigid discipline of his German schoolmasters in Milwaukee remained with him throughout his life. He had full command of Greek and of Latin so that he read the classics in these languages with ease. He would even pick up a modern Greek newspaper and work his way through a paragraph. The chief European tongues of today were at his command. In consequence he could read almost anything that seemed worth while. He was as familiar with Aristophanes as with Aristotle, and what was true of Greek was also true of Latin, French, German, and English. A passage from a modern Spanish novel was once read to him in English translation. He took it down at once to reread it in the original, for he was certain it would have a finer turn in the Spanish phrasing than in the English translation. His hours of reading were any spare time. When a close companion happened to be away with him at a scientific meeting or other such gathering and occupied the same room or an adjoining one, Wheeler would often wake at some such hour as six, call to hear if his companion too was awake, and if so, he would begin reading aloud at this early morning hour from some volume he had near at hand. Thus an unusual moment was made to serve his purpose. Notwithstanding his statements to the contrary, what he read he remembered, but he did not hesitate to reread books many times. It

has been well said of him that he was "possibly the most widely read member of his university," for his reading included belles-lettres as well as science, in short all literature.

As a result of his early, excellent training in languages and of his wide acquaintance with letters, his own style was unusual in clarity and literary flavor; to quote again from a recent appreciation of him, his writing had "a force and a polish, not to mention other qualities, that recall Voltaire. His printed contributions to his subject will perpetuate his scientific memory, and his less technical writings will be read with interest and amusement for a long time to come."

Wheeler's reading and reflection led him to approach special biological problems with a breadth of view not always shown by his colleagues. He was well versed in the history of his science and he was fully aware that that science and in fact science in general was no longer the handmaid of philosophy. Science in itself was to him a growing and gradually all-pervading system of philosophy. This view is now so generally accepted even by the modern philosopher himself that he has given up the invention of systems and shapes his conceptions on what science is gradually discovering. He no longer constructs frames into which science is supposed to fit. Wheeler was perfectly at home in this concept and did his share as a guide to philosophic thinking. He knew the systems of the past as did few others, even the professed philosopher, and yet he was not overawed by them, but chose to dissect them and adopt from them in an eclectic way what they seemed to contain for the present. His truly remarkable acquaintance with what had gone before as well as his unusual linguistic attainments made it possible for one of the wisest of our living thinkers, Professor Whitehead, to characterize him as the only man he had ever known who would have been both worthy and able to sustain a conversation with Aristotle. Yet, as has been pointed out, Wheeler was always soundly scientific in that he relied on the gathering of rigidly controlled observations and the consolidation of these into consistent general concepts as the basis for a universal understanding. Such general views as he held came naturally to him from biological fields and were the outcome of research in these realms. Well he knew the uncer-

tainties and hazards of this kind of occupation, but at no time did he even falter in his belief in ultimate conquest by the scientific method.

One general problem that keenly interested him and on which he repeatedly wrote was that of organic evolution. To him the history of organisms had as much significance for their understanding as their immediate activities had. In this he strongly opposed many in the modern school of general physiology and particularly his old associate Jacques Loeb. Intense and heated were their discussions on this topic. So far as organic evolution is concerned, Wheeler accepted without reserve the importance of Darwin's Natural Selection, but he was no Neo-Darwinist and he never ceased to maintain that the Lamarckian Principle had not been really disproved. It might still have much in it that was worthy of serious consideration. For evolutionary projects and speculation such as these, Loeb and his associates had little or no use and yet the trend of modern biology, much as it has been directed and shaped by these physiological workers, seems now to be turning toward Wheeler's position.

To those who knew Wheeler personally, he was a quiet, modest, unassuming man, the last in the world to reach for distinction and yet happy in its reception. Nevertheless he could be roused to passion, even to strong passion, particularly when the situation seemed to him to carry with it injustice, covered deceit, or insincerity. To none of these indirections would he yield a point and friend or foe must answer him in the open. Yet this passionate side of his nature was not shown to all. In his sketch of Carl Akeley, already quoted, he remarks, "I cannot recall that we were ever even on the verge of a quarrel," and there were many whose personal relations with him were never disturbed by so much as a ripple. Wheeler is too near the present generation to allow any one to form an estimate of his genius, for genius he had in the fullest sense. As a man of scientific letters he was supreme. He was possessed of extraordinary knowledge. He was worthy of all and more than all the distinctions that came to him. His sincerity was beyond reproach. To paraphrase from a recent tribute to him, he was a great experience in the lives of those who knew him and his departure leaves a void that nothing can fill.

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For the preparation of the following list of publications by Professor Wheeler, the author of this Biographical Memoir is under obligation to Professor C. T. Brues and Professor F. M. Carpenter.

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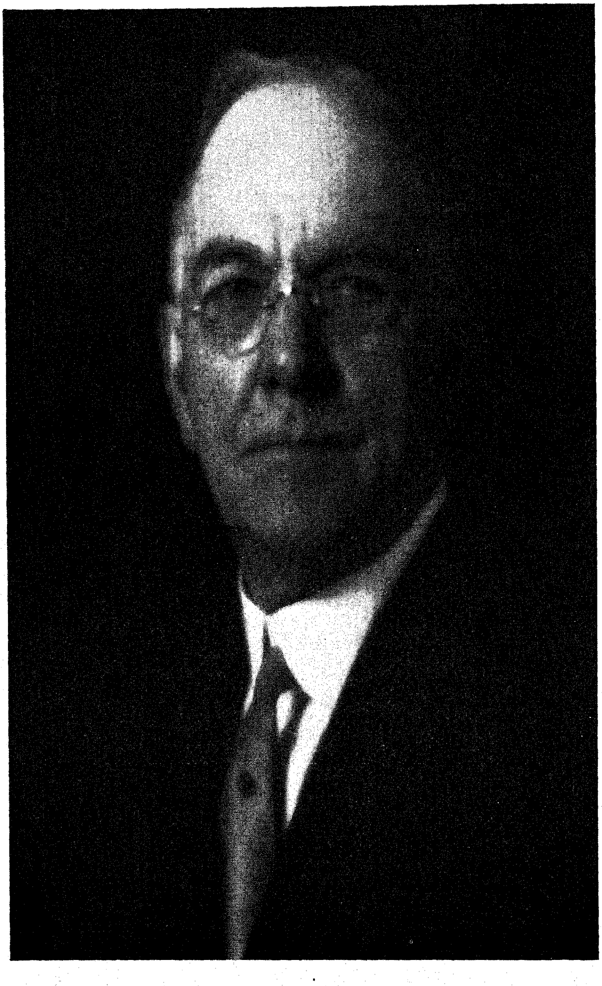

Marshall Avery Howe

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OF

MARSHALL AVERY HOWE

1867–1936

BY

WILLIAM ALBERT SETCHELL
With bibliography by
JOHN HENDLEY BARNHART

PRESENTED TO THE ACADEMY AT THE ANNUAL MEETING, 1938

MARSHALL AVERY HOWE

1867–1936

BY WILLIAM ALBERT SETCHELL

Marshall Avery Howe was born at Newfane, Vermont, on June 6, 1867, and departed this life at his home in Pleasantville, Westchester County, New York, on December 24, 1936, in his seventieth year. At the time of his passing he occupied the position of Director of the New York Botanical Garden.

Marshall Avery Howe was the eldest of five children of Marshall Otis and Gertrude Isabelle Dexter Howe, both parents belonging to old Vermont families of English origin, remarkable for their longevity, their intellectual interests, and, particularly in the case of both his father and mother, for their scholarly inclinations and accomplishments. Both father and mother seem to have been interested in botany, among other nature subjects. Both parents seem to have been of that student and reading class, many of whom existed in rural New England even beyond the middle decades of the Nineteenth Century and whose influence for healthful living and logical thinking had its lasting effect upon their progeny and communicated itself to the community. Marshall Avery Howe was named after his father, Marshall Otis Howe (1832-1919) and his maternal grandfather, Avery Joseph Dexter (1818-1893) and was ever particularly insistent on the use of his full name, which is thoroughly distinctive.

With such parentage and ancestry, it is little wonder that Marshall Avery Howe not only showed forth early his propensities for study, but we can well believe that he received both sympathy and practical encouragement from his parents as well as the esteem and admiration of his fellow townsmen. Since the details of parentage, early education and influences are of such importance in the rounding out and shaping of a career, it seems best to quote several paragraphs of a letter from his brother, Professor Clifton Durant Howe, Dean of the Faculty of Forestry of the University of Toronto, who has kindly supplied them for the information of the writer of this memoir.

"First, in regard to our background, I know hardly anything about my father's ancestors except from general knowledge. I think he was a mutant. His father was a farmer for most of his life; in the earlier part of his life he was a fuller. From the few things I remember my father saying, it is probable that his mother had greater mentality than his father. My father had two sisters and one brother who lived to be in the late twenties and thirties, but who were not outstanding in any way, in fact they all three died of what was then called 'quick consumption.' My father also contracted the disease and was given only a few years to live by his doctors. He had prepared himself for the law and was nearly ready to be admitted to the bar when he began to have hemorrhages. The doctor told him the only chance he had of living, and that for only a few years, was to farm. He married a strong healthy woman and had five boys, all robust and healthy and Marshall's death was the first among them.

"My father lived on a farm all his life but I can hardly say that he got his living by farming. He wasn't strong and robust enough to eke a living out of one of those stony New England farms. The Yankee trading instinct was fairly well developed in him. He made more money buying and selling live stock than he made from agricultural crop production. At one time he even bought raw furs from the trappers in the county. For thirty years or so he was agricultural editor of the local rural paper, furnishing a column every week, and I think got originally two dollars and a half for it and in the end he may have got up as high as five dollars. He wrote particularly upon the trend of agriculture in Vermont, a downward trend which began just after the Civil War and has continued ever since. These articles attracted a good deal of attention outside the state and he was asked to contribute, I remember, to the New England Homestead and to some of the New England magazines. He always took a great interest in education and was superintendent of the schools of the township for twenty-five or thirty years. He was the old-fashioned type of naturalist, interested in all forms of life. He collected plants, specialized particularly upon the grasses and sedges. I can remember that he sent some speci-

mens to old Sereno Watson at Harvard who did not even place his plant in the right genus. My father kept at him until he acknowledged his mistake. He knew all the minerals and collected them. We still have several barrels of his minerals in the barn at the old farm, but the labels have been lost and they are not good for much now. He organized an agricultural library in Newfane village which later grew into a very fine town library. Although he never made any money farming, himself, he knew how it should be done and was the first man in his community to spray his potato vines. He introduced a leguminous crop in the rotation. He also introduced the Jersey cow into the community. He practised forestry in his woodlot except that he did not know it by that name. As boys we were not allowed to cut any straight growing young trees, we were not even allowed to put a straight pole on the old-fashioned pole fences. His care of the hundred acre woodlot served a good purpose, for the revenue from the sale of it, when he became too old to work, supported him for quite a number of years. When I was talking about my father's health as a young man I intended to add that he died in his eighty-seventh year.

"I think, of course, that my mother was an exceptional woman. She taught a country school before she was sixteen years of age. When she was very young, about twenty years old, she made a herbarium of local plants. I still have it and it is in fairly good condition. At intervals during her youth and early married life she contributed short poems to the local papers, all rather philosophical. She organized a circulating magazine club and a discussion club among her neighbors. I can remember one winter when the women met at our house to study some of Shakespeare's plays and at another time Browning's poems. My mother was in her eighty-sixth year when she died.

"Marshall was the first born and was always precocious in his school work. He completed his matriculation Latin and his mathematics by the time he was fourteen years old and this was done mostly in the public school. In those days many of the public school teachers were part way or all the way through college. I can distinctly remember Marshall trudging off to the village on a winter's night, after the farm chores were done, to

recite Latin lessons to the district school teacher. I can also remember my mother drilling him in conjugations and declensions although she was mostly self taught in Latin. He taught his first school before entering high school. I think it was the winter before he was sixteen. He never really went to high school, as we understand the term now, but to one of those good old New England academies or seminaries at the present time nearly extinct. I do not think he spent more than two years actual time in one of those. He taught the winter term in the country schools to earn money for his board and tuition. The principal of the Glenwood Classical Institute of West Brattleboro, from which Marshall graduated was a very scholarly man and he took Marshall far beyond his matriculation requirements in nearly all his subjects. He thought so much of Marshall's attainments that he persuaded him to go back to him to teach Latin for a term during his sophomore year at the University of Vermont. I think Marshall was then in his twentieth year. Although the principal of the school was very fond of Marshall and tried to keep him, his influence must have been towards the study of the classics rather than botany.

"I really think the greatest influence was due to his lifelong friend, fellow-townsman, and fellow-classmate at the University of Vermont, Dr. Abel J. Grout. I think that Abel got interested in mosses before Marshall had thought of specializing in anything. When he did get down to this, he took up the Hepaticae so that he and Abel could work as closely together as possible. The Hepaticae were Marshall's predominant interest when he went to California and, as you know, his first considerable publication was concerned with them. I think his interest turned to marine algae while he was in California. Anyway, I can remember that he began collecting at that time.

"Naturally I feel that Marshall got his real start from the chromosomes that he inherited from his parents and to their continuous encouragement and example in guiding him in the paths of natural history.

"In reading over this letter after it was typed I perceive that I have ignored some of your suggestions. First, in regard to Marshall's early interest in botany, I can hardly remember when

he was not 'puttering' with plants. It certainly began before he left the public school. I can remember his using the oven in the summer kitchen stove in which to dry his 'blotters' and that he changed them very frequently to prevent the discoloration of the plants. Even as a boy he made complete and neat mounts."

Marshall Avery Howe was graduated from the University of Vermont in 1890 with the degree of Ph.B. He, like Sabbatini's hero, Bellarion, had no Greek, so he was constrained to follow the "Latin Scientific" course. He took the Freshman Latin Prize, was awarded distinction in Latin, French, chemistry, physics, mathematics, and zoology. He was elected "Class Leader" by his fellow students. Dr. Abel J. Grout, his roommate and chum, has spoken feelingly of these years in his sketch in "The Bryologist" (40:33-38, 1937, with portrait).

From both Dr. Grout and Marshall's brother, Clifton, it is evident that George H. Perkins, Professor of Natural History in the University of Vermont, must have had a most favorable influence on shaping the trend of the biological activity of both Marshall and his friend Grout. Professor L. R. Jones did not arrive at the University until Marshall's senior year.

On leaving college, Marshall Avery Howe taught almost a year in the Brattleboro High School, but was called, in the late summer of 1891, to the University of California at Berkeley, as Instructor in Cryptogamic Botany. The selection of young Howe was due to Edward Lee Greene, the Professor of Botany and "Head" of the Department. The steps leading to this selection must be interesting, for Greene was not in botanical sympathy at that time with most of his university colleagues of the country, but, at present, those steps are not clear. Young Howe apparently was not particularly happy during the earlier of the five years he spent in Berkeley, but was becoming reconciled to it about the time (1895) that Greene decided to leave the University of California for the Catholic University at Washington. The successor to Greene at the University of California (the writer) was a cryptogamic botanist, thus making two in the same general line at the University of California, and, as it did seem necessary to represent other divisions of botany, Howe resigned

at the end of the 1895-1896 college year to accept a fellowship in Columbia University of New York City.

The period of sojourn in California induced a series of lines of specialization on even such a conservative and deliberate New Englander (and a "Vermonter") as young Howe. His ancestry, working under such an environment as surrounded the brilliant, although opinionated and erratic Edward Lee Greene, put into expression his first dozen botanical articles of one type or another. Howe, even when younger, was slow and deliberate, although articulate and clear. Greene believed in publication, as well as preparation for, and planning for, publication. For *Pittonia* and for *Erythea,* journals conducted by, or founded by, Greene, Howe began his articles, notes, and reviews, having to do largely with Hepaticology (or Bryology) and the suggestions, or at least the influence, of Greene may be seen in the three "Chapters in early history of Hepaticology" (Erythea, 1894-1895). When Howe left California he took with him a very considerable collection of Californian Bryophytes for his future work. He had also made a beginning in Phycology, the other and perhaps preponderant specialty of his professional life. His paper entitled "A month on the shores of Monterey Bay" (Erythea, 1893) contained a report on, as well as a list of, the marine algae he collected, named after consultation with W. G. Farlow of Harvard and the Californian phycologist, Dr. C. L. Anderson of Santa Cruz, California. This "month," as well as other experience of marine algae, was prophetic, but Howe left California to continue his studies in Hepaticology with L. M. Underwood at Columbia University, and continued with certain of the groups of Hepaticae until the end of his life, but his phycological interests recrudesced after his connection with the New York Botanical Garden (1901) and his most distinctive botanical trend was with the marine algae.

Marshall Avery Howe received the degree of Ph.D. from Columbia University in 1898, and from 1898 to 1901 he was a Curator of the University Herbarium, at a time when the efforts of Nathaniel Lord Britton (Professor of Botany at Columbia) for creating the New York Botanical Garden in Bronx Park were materializing. In 1900, the main building of the Garden

was available for use and the herbarium of Columbia University was deposited in it. In 1901, Howe became a member of the Garden scientific staff and in 1906 was advanced to a full curatorship. From 1901 until his death in 1936, Howe was definitely associated with the New York Botanical Garden, being appointed Assistant Director in 1923, and finally, October 1, 1935, he was made Director, after the resignation of Elmer Drew Merrill. It now remains to outline as much as possible of his very varied and effective activities during his 35 years of official connection with the New York Botanical Garden, as collector, arranger of numerous exhibits, morphologist, taxonomist, and distributor of algae and hepaticae; likewise as editor, administrator, and expert and cultivator of dahlias and other ornamentals as educational exhibits. Much may be said, and with profit, about his activities along each and every one of these lines.

The New York Botanical Garden was organized to assemble as much as was possible and profitable of plants, both living and preserved either in proper liquids or dried. Much collecting must necessarily be done and the plants properly prepared for greenhouse, garden, museum, or herbarium, not neglecting material for exchange with other botanical institutions. Members of the staff, specialists, proceeded to various portions of the Atlantic seaboard and the West Indies (Britton's particular interest) for exploration of their floral content and for the critical study of, and report upon, these. The scheme proved effective and, from comparatively small collections, the institution has increased to one whose resources along plant lines are commensurate with the largest and best of its kind.

Beginning with his official connection with the New York Botanical Garden in 1901, Howe entered upon a full life, more intensive in field activities, in the preparation of museum and herbarium material, but continued to issue reports on the material assembled, at first chiefly general, later more and more special, as the algae of the explorations were worked over, the specimens distributed into the herbarium and museum, and the duplicate numbers sent out in profitable exchange.

It is not necessary to follow Howe in all his expeditions to

procure algae for the New York Botanical Garden and for distribution to other similar institutions, but some of the principal ones may be mentioned with profit. In 1900, he visited Bermuda; in 1901 he explored certain coasts of Nova Scotia and Newfoundland; in 1903, the Florida coasts were his objective and were touched again incidentally in 1904, 1909, and in 1914; Porto Rico in 1903 and again in 1915; the Bahama Islands in 1904 and 1908; Jamaica in 1907 and 1909; Panama and Colon in 1910; and Cuba in 1915. From all these and other places, Howe brought back a wealth of algae and other plants for both the material and scientific uses of his institution. Through his recommendation and through the private generosity of Director Britton, various algal herbaria were obtained in their entirety, such as that of Nicolas Pike, containing many specimens early collected from both coasts of North America and from the Island of Mauritius; that of the pioneer phycologist, Dr. C. L. Anderson of Santa Cruz, California, containing much of the California algal flora; and that of Frank S. Collins of Malden, Mass., fairly complete as to New England algae, North American Pacific Coast algae, and a wealth of exotic algae from all coasts of the world obtained by Collins in his exchanges with prominent collectors and phycological authorities of most of the civilized countries. In all, the collections of Algae at the Garden were augmented to 78,229 specimens and those of Hepaticae to 59,240 specimens during Howe's curatorship. The herbarium specimens are accompanied by thousands of microscope slides, prepared in connection with his critical studies, and the exhibit of Algae in the museum is not only extensive but most representative. Many thousands of specimens, most of them with printed labels, were sent out to the leading herbaria of the world and are of the utmost value in following out, critically, his numerous publications.

Marshall Avery Howe did not confine himself solely to the particular groups of plants assigned to his curatorship, but extended his interests and his usefulness to other lines of activity of the New York Botanical Garden. His home interest in ornamentals extended to the Garden. His major project at the Garden concerned the dahlias. Through his energy, care, and

careful planning, there was, during the last years of his life, a splendid collection of these ornamentals grown for display and horticultural study. The stands were excellent. Many horticultural varieties were represented, carefully named, with sources indicated, and they were effectively grouped. The autumnal display of dahlias was one of the popular and professional features of the Garden. Along with this went continuous publication of various papers, to extend the knowledge of varieties and methods of culture and care of dahlias.

Another series of "duties" which came with the curatorship was taking over the matter of editing and providing copy for the bulletins and reports of the Garden itself, or concerning some one or other of its features, for outside information. It seems that Howe was admirably adapted for this type of work, and from 1924 to his death he was editor of the *"Journal of the New York Botanical Garden."* His services were also called upon by the Torrey Botanical Club, and he was editor of its journal, *"Torreya"* and of its *"Bulletin of the Torrey Botanical Club"* for some time. He wrote many reviews of current literature in connection with these duties. His editorial and publicity work came in particularly in connection with the increasing executive work for the Garden, until he was finally definitely appointed Assistant Director (1923) and ultimately Director (1935).

All through his scientific and administrative career Marshall Avery Howe continued his research work, which falls principally under three heads: Hepaticology; Phycology, including the work of the calcareous Algae; and finally, ornamental plants, most largely dahlias. At first Howe was more generally Cryptogamic, and, although one finds nothing regarding Fungi, even in his earliest papers, Ferns, Mosses, Liverworts, and Algae were all passed under review. At the time of his leaving California there was strong impression that the Liverworts were to be his specialty and that seemingly led him to Columbia University and to Lucien Marcus Underwood. He retained his interest in this group, more particularly in the Ricciaceae and the Anthocerotaceae, until the last, but his main effort was more largely devoted, soon after he had finished up his monograph of

the California Liverworts (his doctor's thesis) to the marine Algae. In the latter years of his life, the dahlias came in and he was most active in their connection. By this time also, increasing administrative and editorial work must have cut seriously into his time and energy.

Hepaticology, as has been noted above, was Howe's earliest specialty, although his very first paper (1892) dealt with two Pacific North American Algae (*Fucus evanescens* Ag. and *Gigartina radula* Ag., together with notes on an abnormal form of a grass from Vermont). His most ambitious work on Hepatics was his "The Hepaticae and Anthocerotes of California" (1899), a work based on his five years of collecting in California and attendant and subsequent study. In this work are to be recognized the Marshall Avery Howe characteristics of careful and complete studies of morphology, taxonomic identity, as well as independence, and free discussion of relationships. The descriptions are drawn up by Howe in the full and extended fashion always employed by him and partly due to his own thoroughgoing fashion, possibly, in part, a reflection of the phytographic ideas of E. L. Greene, but more probably representing the meticulous ideas of the author himself. Howe believed in detailed descriptions, which became long, but never involved, and he continued this practice throughout his career.

After the publication on California Hepatics and the beginning of the papers on Algae (1901), the latter activity usurped the field and Howe retained chiefly the Ricciaceae, Sphaerocarpaceae, Riellaceae, and Anthocerotaceae, some of which he continued in cultivation, finally presenting some of these families (partly in collaboration with Caroline Coventry Haynes) in monographic form to Britton and Brown's "North American Flora" (1923). He still continued to be the leading authority in these families and contributed sporadically to the furthering of our knowledge concerning them. He also described (1922, in connection with Arthur Hollick) a presumably fossil Hepatic, under the name of *Jungermanniopsis Cockerellii,* from a Miocene shale in Colorado.

Marshall Avery Howe will, however, chiefly be remembered and estimated as a phycologist. Passing over the two earlier

papers dealing with certain of the marine Algae of California. the beginning of the new outlook came in 1901, when fresh from meeting the representatives of the marine flora of West Indian affinities at Bermuda (1900), he began with a paper dealing with certain of the calcareous green Algae (*"Acicularia"* and *"Acetabulum"*) of that flora. After exploring farther into the Florida and West Indian waters, he published more papers on the calcareous green Algae as well as some on non-calcareous Algae of this and other groups., Finally there came the papers (in connection with M. Foslie) on the "Nullipores" proper, or crustaceous calcareous red Algae, together with insight into their function in reef building, contributions towards our knowledge of marine sediments, etc. The ultimate result of this experience was the identification of certain of them as components of tertiary rocks in the Canal Zone (1918), on certain of the West Indian Islands (1922), from California (1934), and even from the Jurassic rocks of Montana (1925).

Marshall Avery Howe's studies on the marine Algae of the West Indian Islands, although not representing all that he published on marine Algae, stand with those of other writers as preeminent. He added much to our knowledge of the morphology, development, and taxonomy of all the groups of marine Algae and much elimination of confusion was accomplished by his critical examination of algal specimens during a trip to the older herbaria of Europe (1904) for the purpose of studying the type specimens of American marine Algae therein preserved. From the extensive notes of his studies and about 300 photographs to show habit, Howe was in a far better position to correlate his own studies on the shores, with what had been written or postulated by earlier and contemporary writers than were any of his predecessors. He certainly made full use of these unusual facilities and was a prominent factor in bringing order out of chaos throughout the extensive and most important West Indian marine flora.

In the West Indian marine flora, Marshall Avery Howe found the principal motive of his research work. The Caribbean Sea is a closed area of warm tropical and subtropical waters, characterized by its abundance of Corals and calcareous Algae. It

is a relatively shallow basin, largely segregated from the Atlantic by an island chain (the Greater and Lesser Antilles) ; from the Mediterranean, with which it has affinities, by the breadth of the Atlantic Ocean; and from the East Indian seas, with which it also has affinities, by the central American land mass and the Isthmus of Panama, as well as by the northern breadth of the Pacific Ocean. In many ways, it recalls, as do also the Mediterranean and East Indian seas, the conditions of the ancient Tethys. Howe realized more and more the especial problems of this remarkable body of water and, as his experience and knowledge of its calcareous marine Algae developed, he began to see more and more the relations between them and world problems. To many of us, the contributions towards solving the problems of the calcareous Algae were the most important of Howe's scientific life. A brief résumé of these may therefore be attempted, to elucidate his attitude toward them.

Among the calcareous Algae, so abundant on the coasts of the Caribbean Islands, Marshall Avery Howe first attacked those belonging to the Chlorophyceae (or green Algae). He most thoroughly investigated the West Indian species of *Acicularia, Acetabulum* (*Acetabularia*), the variable and troublesome species of *Halimeda,* and of *Neomeris.* Almost of necessity he made studies of the other complex *Siphonales,* which are little or not at all calcified. The contributions made to our knowledge of their vegetative structure and reproduction shed much light on their taxonomic relations, both within the genera, within the families, and within the order.

The second great group of calcareous Algae, occurring in profusion on the reefs of the Bahama Islands in particular, is that of the Crustaceous Corallines, a group of the red Algae, or Rhodophyceae. In the beginning he enlisted the collaboration of M. Foslie of Trondhjem, Norway, the great authority on these plants, but while Foslie may have been largely responsible for the final determinations, it was certainly Howe who not only made the collections and selected the specimens for study, but also prepared the sections which were used as the basis for making clear their structure and relationships. Foslie and Howe

published two papers (1906) on species from Florida, Bahama Islands, and Porto Rico. These publications, besides the excellent descriptions and discussions, were provided with a most excellent series of illustrations, both of general habit and of what is almost unique in the exposition of these plants, extraordinarily effective reproductions of photomicrographs of sections. Through these illustrations of microscopic structure a new era was instituted in the more exact study of the crustaceous corallines and the more massive of the Caribbean species were the first made available for effective comparison with other tropical species.

Through these studies and others not so definitely published, Howe became interested in two phases of the relation of the crustaceous corallines to world problems; viz., the relation of the crustaceous corallines to the building up of the huge structures known as "coral reefs," and the relation between the recent living crustaceous corallines and those, now extinct, of previous geologic ages.

Concerning the first, in 1912, he published a much needed discussion of the subject in "*Science*," on "The Building of 'Coral' Reefs," in which, reviewing the general subject, he concludes that "the importance of the corals in reef-building has been much overestimated and that the final honors in this connection may yet go to the more humble lime-secreting plants."

On the fossil crustaceous corallines, he published several papers (1918, 1919, 1922, 1925, and 1934), in which his extensive knowledge of the living forms enabled him to interpret accurately their fossil representatives. From the marine crustaceous coralline and its relation to rock building came easily an interest in fresh water forms associated with the formation of travertines. He made some studies and many examinations of both recent and fossil material. Two publications (1931, 1932) resulted and more might have been expected. An appreciative note on his assistance in solving geological problems has been published since his death by T. Wayland Vaughan (Marshall Avery Howe, Jour. Paleont., 11, no. 4: 368-370, June, 1937) with pertinent bibliography (by Rosalie Weickert).

Of the calcareous reds other than the crustaceous corallines and as occurring in the Caribbean Sea, Marshall Avery Howe made many studies, the taxonomic being recorded in his various floras, but of the species of two genera he worked out and reported some fundamental facts leading to profound changes in our attitude towards their life histories and relationships. In 1917 and 1918 he brought forward facts and experience to indicate a structural, sexual dimorphism among species of *Galaxaura,* the adult alternative generations differing sufficiently to have been classified under different subgenera. In 1920, he published a detailed paper on the "monosporangial discs" of various species of *Liagora,* a new type of structure which had barely been mentioned previously, but which Howe suggests may be integral parts of the life history of the plants on which they occur rather than independent or obligate epi-endophytes.

Much more might be said of the strictly scientific work of Marshall Avery Howe, but suffice it to say that it was done meticulously, with insight, and thoroughness. One may turn to his application to his other duties, both as an official of the New York Botanical Garden and as a citizen. As administrator, his success was sufficient to cause him finally to be appointed to the directorship. On the horticultural side, he not only cultivated ornamentals in his own garden but instituted a display of dahlias at the New York Botanical Garden which was internationally famous. A glance through the appended bibliography will indicate the more than "amateurish" interest he took in this activity. He was a charter member of the Vermont Botanical Club (1895). At Pleasantville, N. Y., where he made his home for 22 years, he performed his duties to his fellow citizens as an active member and sometime president of its "Garden Club"; and served as secretary and later as president of the Board of Trustees of the Pleasantville Free Library. He was awarded a gold medal by the American Dahlia Society, was a Vice-president of the Associated Garden Clubs of New York, and was a member of the Board of Directors of the Horticultural Society of New York.

In recognition of his scientific work, he was awarded the

honorary degree of Sc.D. by his *alma mater,* the University of Vermont, in 1919, and he was elected a member of the National Academy of Sciences in 1923. He was a Fellow of the New York Academy of Sciences, which he served as President in 1934 and 1935. He was Vice-president of the Botanical Society of America in 1913 and was elected President for the year 1937, too late to be able to serve. He was also President of the Torrey Botanical Club for the year 1936.

His home life was quiet and dignified. At home in Newfane he was very busy with chores about the farm, his earlier intervals of evening teaching for board and tuition, and his botanizing and preparation of herbarium specimens. On June 8, 1909, he married, at Stratford, Conn., Edith Morton Packard, who passed away October 18, 1928, after some years of lingering illness. Two children, Gertrude Dexter Howe and Prentiss Mellen Howe, survive their father.

Marshall Avery Howe was of the older as well as one of the present generation. He began his vocational work in early childhood. He was a real "naturalist." Yet, as he later developed, he fell into the swing of modern ideas, still attending to many subjects but doing each well. His love for Hepaticae, his devotion to the Algae, and his successful work with his dahlias, all redound to his credit and indicate his thoroughness, his precision. His calm yet full-hearted enthusiasm, coupled with persistence and skill in communicating his results to others, were his especial and most admirable characteristics.

In preparing this memoir, use has been made of the following:

Marshall Avery Howe (Jour. N. Y. Bot. Garden, 38, no. 446: 25-31, Feb. 1937) by A. B. Stout.

Marshall Avery Howe (Jour. Paleont., 11, no. 4: 368-370, June 1937) by Thomas Wayland Vaughan (with bibliography on Calcareous Algae by Rosalie Weickert).

Marshall Avery Howe, 1867-1936 (Bryologist, 40, no. 2: 33-36, Mar.-Apr. 1937, with portrait) by A. J. Grout.

Much information was gratefully received from Professor Clifton Durant Howe of the Faculty of Forestry of the Uni-

versity of Toronto, one of the four surviving brothers of Marshall Avery Howe, and from Dr. Abel J. Grout of Newfane, Vt., and Manatee, Fla., Howe's close associate and roommate in college.

For the appended bibliography, all credit is due to Howe's associate for many years at the New York Botanical Garden, Dr. John Hendley Barnhart.

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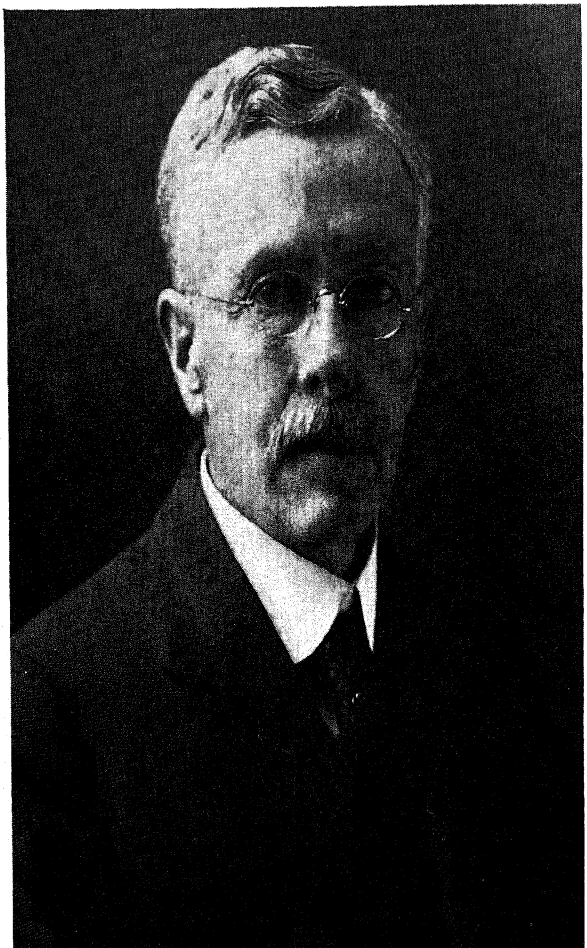

H. P. Armsby

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OF

HENRY PRENTISS ARMSBY

1853–1921

BY

FRANCIS G. BENEDICT

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HENRY PRENTISS ARMSBY

1853–1921

BY FRANCIS G. BENEDICT

"Born of good, solid New England stock," an expression characterizing so many of America's foremost citizens, applies to Henry Prentiss Armsby, who, the only child of Lewis and Mary A. (Prentiss) Armsby, was born September 21, 1853, in Northbridge, Massachusetts. The father, a skillful cabinet maker, early moved to Whitinsville and subsequently to Millbury, Massachusetts. After receiving the usual elementary school training in these towns, Henry entered the Worcester County Free Institute of Industrial Science at the age of fifteen years. This Institute later became the Worcester Polytechnic Institute. The records show that Armsby, as a schoolboy, was "absorbed" in chemical experiments. Three years later (1871), in the first class to graduate from this Institute, he received the degree of Bachelor of Science. The following year he remained at the Institute as instructor in chemistry and then realized the objective of many years, graduate work at Yale University under the inspiration of Professor S. W. Johnson. Receipt of the degree of Bachelor of Philosophy at Yale University in 1874 was coincidental with his first scientific publications, "On the Nitrogen of the Soil" and "Experiments on the Decay of Nitrogenous Organic Substances." After a year of teaching science in the high school at Fitchburg, Massachusetts, a long-wished-for sojourn in Germany (Leipzig) followed. Here, under Gustav Kühn, in company with his close friend, E. H. Jenkins of New Haven, a most profitable year full of intense work followed. Jenkins comments on Armsby's method of work as follows:

"He was a thinker. . . . He would sit with his feet to the fire, not drowsing or dreaming, but thinking hard, and then he would go to his desk and write awhile, and then come back to think."

This habit of intense, concentrated thinking characterized the man's entire life. A snap judgment, a hunch, seemed out of his experience.

Although trained as a chemist, Armsby, in the Kühn atmosphere at Leipzig, began to take special interest in problems of physiology and from then on drifted more and more towards physiological work. This inclination culminated in his classic contributions to animal nutrition and calorimetry. Nevertheless he was destined to live a chemist's life for some time, for upon his return from Germany he taught chemistry at Rutgers College, New Brunswick, New Jersey, from 1876 to 1877.

The Agricultural Experiment Station movement began under the stimulation of Johnson and Atwater, and in 1877 Armsby was called to Connecticut to be chemist of the first Agricultural Experiment Station established in the United States. Recognizing the paucity of information in English on cattle feeding, Armsby immediately began a translation of E. v. Wolff's "Landwirtschaftliche Fütterungslehre." The changes and additions that he found necessary to bring this up to date for American conditions resulted in his first book called "Manual of cattle feeding," which was published in 1880. A year before this Yale University had granted him the degree of Doctor of Philosophy. His intense interest in animal feeding led rapidly to the publication of a series of papers on the digestion, the composition, and the utilization of feeds by domestic animals, accompanied by a number of papers on the chemistry of soil, milk, and problems dealing with the furthering of agricultural science. His two years (1881-1883) as Vice-Principal and Professor of Agricultural Chemistry at the Storrs Agricultural School (later the Connecticut Agricultural College) were somewhat in the nature of marking time, but in his appointment as Professor of Agricultural Chemistry and Associate Director and Chemist of the Agricultural Experiment Station at the University of Wisconsin in 1883 he received his first great impetus to administrative and research work. Here he remained until, in 1887, he was called to the directorship of the new Agricultural Experiment Station of Pennsylvania State College, where his great life work was to be accomplished.

The fact that his whole life was associated with institutions of agricultural research has to some extent pushed into the background the fact that his interests were far broader than even

the wide limits of the term "Agricultural Research" would imply. Chemist, physiologist, author, critic, administrator, organizer, all combined in a marvelous manner to make every undertaking an instant and permanent success. The administrative work was irksome but faithfully carried out, and it was with joy that in 1907 he was relieved in part of much routine administrative work as Director of the Pennsylvania Agricultural Experiment Station and made Director of the newly organized Institute of Animal Nutrition of Pennsylvania State College. Although his efforts in behalf of agricultural science prior to 1907 were invariably successful, it was with the new Institute that the full powers of this man developed.

Nearly ten years before, Armsby had become convinced that the true basis of animal feeding, a problem at once of deepest physiological importance and of tremendous economic significance, could best be studied by a wholly new line of attack. Food is given to keep animals alive, to provide for normal growth, to facilitate the best powers of reproduction, and, in the case of cattle, to render fit for human consumption in the shape of beef flesh or milk the fodders, forages, and grasses inedible to man. The energetics of these transformations had always intrigued him and, according to his concept, the one logical method of studying them was with an accurately measurable standard, *i. e.,* the calorie. At the Institute of Animal Nutrition, then, he began plans for that unique instrument, the great respiration calorimeter for large domestic animals. This was housed in a special building, together with most perfect accessory apparatus, and technicians were carefully trained to operate it. Naturally these heroic experiments challenged the attention of research workers in animal nutrition throughout the world. So perfect was the design, construction, and operation of this intricate apparatus that immediately Armsby began to unfold new concepts and impress practices in the highly important economic and physiological problems of animal nutrition. Similar problems had been challenging European scientists for many decades, and shortly his apparatus was duplicated by Hagemann at the Experiment Station at Bonn-Poppelsdorf. Hagemann made a number of unsuccessful "betterments" but was never able to

secure satisfactory results with the equipment, clearly due to his inability to follow well-laid rules and constructive design. A second replica was constructed in the admirable laboratory of Tangl at Budapest. Unfortunately, owing to his untimely death, this instrument was never put into successful operation. Hence today the only respiration calorimeter in which at the same time direct and indirect calorimetric measurements can be made on large domestic animals is that constructed by Armsby, which remains as his monument at State College, Pennsylvania.

Not only was his apparatus at the Institute of Animal Nutrition copied by at least two great European centers of research, but his Institute became a Mecca for a host of scientists who came to this country interested both in animal and in human nutrition, and a visit of Rockefeller Fellows or medical groups from other lands, studying human nutrition in this country, was never complete without a trip to the Armsby Institute at Pennsylvania State College.

In 1905 the Carnegie Institution of Washington commissioned Armsby to examine and report on the entire procedures of the Atwater respiration calorimeter for humans at Wesleyan University, Middletown, Connecticut, preliminary to the establishment by the Carnegie Institution of Washington of a Nutrition Laboratory in Boston. No man in America other than Armsby was even considered for this survey. His critical inspection and suggestions were all presented in a report so complete (as an engineer's report on a project) that the Nutrition Laboratory was started with the sound confidence of its success. In 1919 the Carnegie Institution of Washington again availed itself of Armsby's superior critical judgment, when, prior to the initiation of an extensive study of the basal metabolism and the energy transformations in the complex digestive system of large ruminants, Armsby was commissioned to survey critically the joint research project of the Carnegie Institution's Nutrition Laboratory and the Laboratory of Animal Nutrition (under the direction of Professor E. G. Ritzman) at the New Hampshire Agricultural Experiment Station, Durham, N. H. The project involved the then considered hazardous procedure of submitting ruminants to one or two weeks of fasting (with

water), to observe what the ruminant does with its body reserves, at how low a level of vital activity it can live, and whether upon re-feeding its original metabolic level will be resumed. As a result there was instituted simultaneously at the New Hampshire Agricultural Experiment Station and at Armsby's Institute most profitable researches on the effect of food withdrawal upon the ruminant. From this date onward there was a constant interchange of ideas between the New Hampshire Laboratory, the Nutrition Laboratory in Boston, and the Armsby Institute, all working on closely related problems. This was a striking example of cooperation, with the younger men ever seeking aid from the master. Almost the last letter penned by Armsby dealt with the numerous problems and discussions of the three laboratories. No name appears more frequently in the joint publications of the Nutrition Laboratory and the New Hampshire Agricultural Experiment Station than that of Armsby.

In view of his unique mastery of agricultural chemistry, animal husbandry, and animal nutrition, it is no wonder that his writings were and are today looked upon as authoritative. Through his writings and through his personal visits to Europe, Armsby became well known in all the centers of agricultural research, especially those dealing with animal nutrition in Europe, and no American in agricultural science was more frequently mentioned in discussions than Armsby. His second book, published in 1903, on "The Principles of Animal Nutrition" was in a sense a startling innovation for a man in an agricultural experiment station to write, for it took animal nutrition completely out of the rule-of-thumb barn manual, laid before animal husbandmen the basic principles upon which such feeding must ultimately be considered, and charted many of the guiding courses for his subsequent researches in calorimetry. Fourteen years later his book entitled "The Nutrition of Farm Animals" appeared. This book incorporates most successfully deep scientific insight into the innumerable problems in animal nutrition and a great deal of practical knowledge, which makes it an admirable basis for much agricultural nutritional teaching and likewise the basis for much research. All those interested

in animal nutrition have found a wealth of material in this most carefully prepared, comprehensive work. Concurrent with his important advice on the nutrition of man and animals at the time of the World War, there appeared his book on "The Conservation of Food Energy," which made a distinctly favorable impression. This was almost simultaneous with his appointment as a member of the Interallied Scientific Food Commission, upon which he remained until the end of the war. A posthumous book, the details of which had been carefully worked out prior to his death, was left for completion in the capable hands of the junior author, Professor C. Robert Moulton, and appeared in 1925 under the title "The Animal as a Converter of Matter and Energy. A Study of the Role of Live Stock in Food Production."

Armsby was married to Lucy Atwood Harding of Millbury, Massachusetts, on October 15, 1878.

The honors conferred on Armsby, both at home and abroad, were modestly received but invariably increased his sense of responsibility and obligation. The most outstanding of these honors were:

Doctor of Laws, University of Wisconsin, 1904
Elected member of the Royal Academy of Agriculture of Sweden, 1912
Doctor of Science, Yale University, 1920
Elected member of National Academy of Sciences, 1920
Doctor of Science, Worcester Polytechnic Institute, 1921

Armsby developed during that period when agricultural chemistry, as W. H. Jordan wittily remarked, was so looked down upon that the wife of the professor of organic chemistry occupied a distinctly higher social level than the wife of the professor of agricultural chemistry or, as it was often irreverently called, "cow chemistry." One often wondered if Armsby sensed keenly this lack of appreciation of the importance of agricultural chemistry and by his personality did all he could to overcome this injustice. When one met this gentleman his graceful, modest bearing, his courteous manner, his meticulous dress, instantly dispelled all erroneous notions of the standard of deportment and appearance of the agricultural chemist. A

striking feature of Armsby's personality was his invariably well-groomed appearance. He was a most modest man and spoke at formal or social gatherings with a delicacy of expression, a dry wit, and a charming manner that were the envy of his colleagues. Although having too few hobbies and relaxations, he enjoyed especially bridge whist, to which he applied his mathematical mind and prodigious memory to such purpose that it was commonly said that after the second hand was played he knew the location of all the remaining cards. Because of his naturally frail constitution he felt the responsibility for exercise and was the first golfer on his campus, driving the ball before him in his trips each day to and from his laboratory. His "one man golf club" remained an institution for years. A man considered by most as reserved, although by no means unapproachable, Armsby was, to those fortunate enough to be accepted as intimates, a rare personality. Full of courtesy, dignity, and charm, he became in the intimate circle an endearing personality, rich in dry humor and anecdote. At the only annual dinner (1921) of the Academy that he could attend, those who were at table with him will not forget his clever and delightful anecdotes. The writer was the only one of his "Fach" in the group, and it was interesting to note how he held the attention of those eight others, who represented different branches of science.

His appearance of reserve was undoubtedly due to the fact that he was completely absorbed in his work. There was too little of interest outside the laboratory, and although he took his civic duties seriously, always being available for university committees, an active church worker, and a senior warden in the Episcopal Church, nevertheless his first thought, after his family, was his laboratory and his science. As Director of an Agricultural Experiment Station, he owed certain duties to his constituency which he did not neglect, and the earnest of this is shown in his innumerable papers and the agricultural reports dealing with practical problems of the farmer. On the other hand, his contributions to abstract science are represented by his scholarly book on "The Principles of Animal Nutrition," his development of the unique respiration calorimeter, and the long

series of fundamentally important researches carried out with this instrument.

With almost pitiless energy he drove his research unendingly, and a familiar picture was he, returning to his home from the laboratory, often with a batch of protocols and a large slide rule under his arm. Although he had perfect confidence in the technical operation of his intricate respiration calorimeter by his highly trained associates, he personally supervised all the innumerable calculations. This of itself was an almost superhuman task, and those of us who knew him intimately often have felt that this pressure was his ultimate undoing. Shuttling between the rather inaccessible State College in Center County, Pennsylvania, and the bureaus in Washington at too frequent intervals undoubtedly was more than his frail constitution could stand, but with that spirit of service that characterized him in all his life, he was unwilling to slack any of the work. Perhaps Armsby's greatest fault was that the word "no" did not exist in his vocabulary. To refuse a request of his colleagues or fellow scientists in other institutions, or to refuse the call of service to the Government was to him unthinkable, and as a result he, with his extraordinary capacity for diplomacy, clear thinking, and wide vision, and above all his successful accomplishments, was called upon too much.

Of the innumerable expressions of appreciation of colleagues, associates, and friends, which followed his death on October 19, 1921, but two will be cited here. Professor W. H. Jordan wrote as follows:

"Armsby was a learned man in nutritional science, one of the leaders in this country, perhaps *the* leader in the nutrition of farm animals. When in Berlin in 1913 I asked Dr. N. Zuntz if the United States had any research workers in animal nutrition of equal standing with the best men in Europe. Zuntz answered immediately, 'Armsby.'"

Professor C. Robert Moulton, his collaborator in his last book, wrote as follows:

"Professor Armsby impressed me as an aristocratic gentleman and a member of the intellectual nobility. He was courteous and dignified, yet he showed a true interest in our welfare. The

operation of his laboratory required a well integrated staff with duties assigned and rehearsed, yet one did not feel that he was a cog in a machine, but an individual that was vital to the smooth working of the project. In so far as was possible he allowed his men a freedom and initiative. They were encouraged to give new ideas and suggestions concerning the work of the Institute. He was generous in his dealings with the members of the staff."

Owing to lack of space, the writer refrains from personal statements, much as he would like to make them, for Henry Prentiss Armsby will always remain as one of the rarest figures in his life.

Acknowledgments. In the preparation of this material I have been greatly aided by one of Professor Armsby's sons, H. H. Armsby, Registrar of the School of Mines and Metallurgy, University of Missouri, Rolla, Missouri, and by Professor Max Kriss of the Institute of Animal Nutrition, State College, Pennsylvania. The bibliography was kindly furnished by Dr. E. B. Forbes, Director of the Institute of Animal Nutrition, State College, Pennsylvania. Material has freely been drawn from the excellent book by Professor Thomas I. Mairs entitled "Some Pennsylvania Pioneers in Agricultural Science," published at State College, Pennsylvania, 1928.

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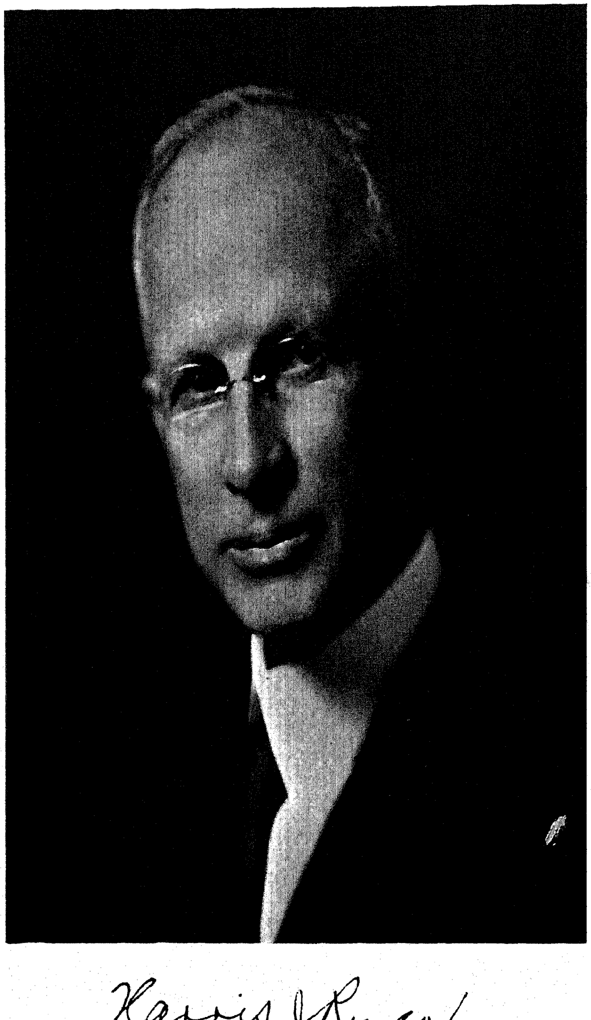

Harris J. Ryan

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OF

HARRIS JOSEPH RYAN

1866–1934

BY

W. F. DURAND

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1866–1934

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The family of Professor Harris Joseph Ryan on his father's side traces back to pre-revolutionary Irish stock in the person of an enterprising lad who, sometime perhaps about the middle of the eighteenth century, came to this country and took up land in Pennsylvania, married and finally settled down in what later became the little town of Matamoras (post office, Powells Valley). The family flourished, and some three or four generations later, on January 8, 1866, there was born to Charles W. and Louisa M. (Collier) Ryan, a son who was christened Harris Joseph. On the mother's side, the Colliers traced back to early Scottish stock which, intermarrying with the German and other early Pennsylvania stocks, furnished much of the sturdy backbone of the rural population in east central Pennsylvania as found there during the nineteenth century.

When Ryan was still a small boy, his father moved from the farm in Matamoras to Halifax where he had interests in lime kilns and later became cashier of the Halifax National Bank, which position he retained until his death in 1901.

Ryan's first schooling was in a country school in Matamoras, following which he passed a few years in the schools of Halifax, and later in a school at Mt. Airy, Philadelphia. These earlier periods of schooling covered what would now be called the "grades," following which he covered the requirements for college entrance, partly at the Baltimore City College and partly at the Lebanon Valley College.

It is always fascinating to speculate on what might have been the future career of a man of mark if, when at some crossroads of life, he had taken a turning different from that actually followed. So here, the natural turn for young Ryan, with college preparatory in Baltimore and vicinity, would have been to plan ultimately for Johns Hopkins (then a post graduate institution only) and this would presumably have been the course followed

but for an accidental meeting with an enthusiastic young student from Cornell at home on a vacation.

The picture of life at Cornell and of the opportunities to be found there proved so alluring to young Ryan that all other plans were put aside and he entered Cornell in 1883.

His interest in science had early begun to appear. There is extant in the family a story of demonstrations which, as a school boy, he carried on with a miniature chemical outfit which his father had given him, much to the interest and wonder of friends and relatives.

There was growing interest likewise in the field of physics, or "natural philosophy" as it was then often called, and with increasing maturity, especially in the domain of electricity which, in the early eighties was just beginning to give some faint indications of the part it was soon to play in our modern civilization. With these trends of interest, it was only natural that at Cornell, his immediate objective would be the course in electrical engineering just at that time announced in the Department of Physics under Prof. Wm. A. Anthony.

In 1926, on the occasion of the award of the Edison Medal to Professor Ryan, referring in his response to this period of his life he said:

"Forty-three years ago this fall I entered Cornell as a freshman to take up the curriculum in electrical engineering, that had just been established and for which students were being admitted for the first time. The electrical engineering laboratory of the University was little more than the electrical section of the physics laboratory of that day. The little more was one direct-current generator invariably referred to as the Gramme dynamo that was built by Professor Wm. A. Anthony, the 1890-1891 President of the Institute.

"Professor Anthony visited France immediately after 1872 when Gramme had completed his direct-current generator, generally conceded to have been the first direct-current dynamo of size adequate to reveal its possibilities in the engineering industries. Professor Anthony visited Gramme, saw his generator, and on returning to Cornell immediately set about to construct a replica thereof. It was completed in 1874 and exhibited just a half century ago at the Centennial in Philadelphia. Curricula in electrical engineering at Columbia, Cornell and other uni-

versities were announced somewhat less than ten years after the Centennial."

In this way the young student Ryan came under the stimulating leadership of Professor Anthony, who, perhaps more than any other of his teachers, exercised a guiding influence into and along the lines of work which later became his chief life interest.

In this connection a letter which he wrote to Mr. F. J. Sprague under date of June 7, 1932, on the occasion of a birthday dinner to Sprague in New York City, is likewise of interest. In this letter he recalls a visit made by Professor Anthony to the Sprague Works in New York in the winter of 1886-87 with a small group of his students, among whom he (Ryan) was present. He then recalls to Sprague that, in the conversation between himself and Anthony, he (Sprague) said in substance, "In my studies I have found that economy in the electrical transmission of power will be directly proportional to the voltage and inversely proportional to the distance." And then continues Ryan to Sprague, "Thus this started me out in life with a never ending enthusiasm for the study of high voltage phenomena."

The Cornell of 1883 was an institution of only slightly over four hundred students. There was only one building available for dormitory purposes and this was mostly occupied by younger members of the teaching staff. Most of the students lived down near the foot of the hill or in the nearer parts of down town Ithaca. Ryan's life at Cornell was much like that of other students of those days. Naturally studious and with his interests continuously stimulated by his environment, he gave himself with enthusiasm to his studies.

During this period of student life he evidently made a deep impression on Professor Anthony as a young man of exceptional promise. This resulted in his being selected by Professor Anthony, in his senior year, as his immediate assistant in much of the special work with which he was engaged in those days. The "copper house" and the great tangent galvanometer therein were for many years landmarks at Cornell, recalling Professor Anthony's pioneer work, especially in connection with the establishment of electrical standards. The copper house and the

tangent galvanometer were built during Ryan's student days and he was privileged to work directly with Professor Anthony, not only in the building and installation of this equipment, but also to share with him in some of his pioneer work in which this equipment played a major role.

So passed the years until 1887, when, graduating with a high record, he was offered an instructorship in physics; but, during his later years at Cornell he had made the acquaintance of two students there for graduate work, J. G. White, founder later of the engineering firm of J. G. White and Company of New York City, and Dugald C. Jackson, later distinguished educator and author in the field of electrical engineering. In these years electrical engineering as a profession was the youngest of the great divisions of engineering work. The American Institute of Electrical Engineers was organized in 1884, the year after Ryan's entrance into Cornell. Nevertheless there were then men of vision who foresaw, even though dimly, some part of the great role which it was destined soon to play in carrying forward the developments of the next half century. White, Jackson and Ryan had such faith and such vision; and White and Jackson, planning for the period following the completion of their graduate work, had gotten from Ryan a promise that he would join them in the formation of a company for the active development of projects in the field of electrical engineering in some promising section of the country.

White, having finished his work in 1886, had taken a teaching position in the University of Nebraska at Lincoln and the following year, in 1887, Jackson and Ryan joined him in Lincoln, and White, resigning his position in the State University, these three young pioneers organized themselves into the "Western Electric Company" and carried on actively in Lincoln and the surrounding territory.

This association lasted for about a year; but in the meantime the call of the class room and of life at Cornell had begun to assert itself with growing strength, and, the offer of the instructorship having been repeated, Ryan returned to Cornell in 1888 and took up his work as Instructor in Physics.

In 1885 Dr. Robert Henry Thurston had come to Cornell

from Stevens Institute, as Director of the Sibley College of Mechanic Arts. Dr. Thurston came to Cornell with large plans for widening the scope of Sibley College, for improving its standards and for making of it an engineering school of the highest standing—plans which in gratifying degree were realized, as witness the reputation which this college has enjoyed in later years. Among his first moves for such widening and improvement was the plan of giving distinct recognition to electrical engineering, and of laying down a distinct course in Sibley College, leading to a degree in electrical engineering, and sufficiently divergent in requirements from those for the course in mechanical engineering, to permit of suitable training in electrical theory and correlative laboratory practice.

By the summer of 1888 the time seemed ripe for a definite move toward the realization of these plans.

As noted earlier, the first announcement of special instruction in electrical engineering at Cornell was in the University Register for 1883-84. In that year the trustees gave formal recognition to the earlier work of Professor Anthony and authorized the faculty to "announce a course of study in electrical engineering leading to a degree". The Register, in outlining this course, refers to the "demand for thoroughly trained engineers conversant with electrical science" and to "special studies embracing the construction . . . of dynamo machines and the methods of electrical measurements, electric lighting and the electrical transmission of power." In accordance with these plans, Professor Anthony continued in the direction and development of this work in electrical engineering until 1887 when he left Cornell to undertake electrical development work along industrial lines. At Cornell, Professor Anthony was succeeded in the Department of Physics by Professor E. L. Nichols, and a year later an Assistant Professor of Electrical Engineering was appointed in Sibley College. By understanding between Sibley College and the Department of Physics, it was agreed that the work in electrical engineering would be divided between the two, the former taking over what might be called the more professional aspects of electrical engineering, while the latter would continue to give courses in the scientific background, in

electrical measurements and in dynamo laboratory practice. This general arrangement continued for some years, with a gradual further shift of the work from the Department of Physics to Sibley College. In the meantime, as noted, Ryan, during the year 1888, held his position as Instructor in Physics, but his work, in pursuance of the arrangement noted above, was almost wholly in electrical engineering subjects.

At the beginning of the academic year 1889, the incumbent in Sibley College leaving to take up other work, Ryan was appointed to his place as Assistant Professor of Electrical Engineering in Sibley College and took up his work in the fall of that year in such capacity.

No attempt will be made at this point to appraise in detail his work during the next sixteen years at Cornell. It was characterized by intense activity, notable achievement, and phenomenal growth in the number of students seeking instruction under him. This was due in part to the rapid growth of electrical engineering in those days, but also in no small part, to the faculty which he had of inspiring and stimulating all who came into contact with him in the relation of pupil and teacher.

In the closing months of 1903, Dr. Thurston, Director of Sibley College, died, and in the spring of 1905 there came from Stanford University, California, an offer to head the Department of Electrical Engineering in that institution. The head of his department in Cornell and a full professor in rank, no prospects could have seemed fairer. Beloved by both students and colleagues and with a distinguished record of success both as a teacher and in his personal contributions in the domain of electrical engineering, there was no reason for making a change beyond what may perhaps be termed the call of the West. Stanford University, California and the wide Pacific, all beckoned, and resigning from Cornell he took up his duties in Stanford in the fall of 1905. Here he remained in active service until his retirement as Emeritus Professor in 1931. Following his retirement, with health somewhat impaired, he spent most of his time quietly at his home in Palo Alto, much occupied, among other things, with studies relating to the deaf and the application of electrical aids to hearing for the alleviation of

this all too common handicap to human activity. He had, himself, in his later years suffered from this limitation, and had obligingly submitted himself as a subject to the successive forms of equipment devised in the research laboratories of the American Telephone and Telegraph Company as aids to hearing. Helped as he was by the improving efficiency of these devices he was anxious that others, so far as possible, should share in such benefits and from this desire came the studies which occupied much of his time in these later years.

However, these studies and other matters on which he was thinking were brought to an end in the spring of 1934 by a cerebral hemorrhage which left him with partial paralysis on one side of the body. Following this came a gradual improvement and he was making fair progress toward a comfortable condition of recovery, when on July 3, 1934, a cardiac weakness placed a period to his life.

Such, in bare outline, are the more controlling features of the life of Professor Ryan.

In approaching some account of his professional and scientific achievements, attention is immediately arrested by the third item in the list of his published papers. It is rare, in the scientific work of a man of mark, that so wide a recognition is gained by his third published paper. Speaking of this period of his life, on the occasion of the award of the Edison Medal in 1926, Ryan said:

"In the fourth year of the Institute [A.I.E.E.] I began my work as a faculty man at my alma mater. I found that I was wholly unprepared to assist my students effectively to an understanding of things without end, encountered everywhere; particularly was this so, as matters stood in that day, for the transformer in the alternating-current circuit and the armature reaction effects in the continuous current machine. The alternating-current system for economic incandescent lighting so well suited for the needs of the new rapidly growing American towns and cities had been introduced three years before, i.e., in 1885-6 and its use was being extended rapidly.

With the aid of a friend of my student days, Ernest Merritt, past president of the American Physical Society, I worked through the summer of 1889 upon the problem of systematic measurement upon a particular transformer in sufficient detail

to meet our requirements for teaching. The work was done at Buffalo, New York, through the courtesy of C. R. Huntly, Executive, and H. H. Humphreys, Engineer, of a lighting company of that city. We selected for our specimen a 10-light, 2000 to 50-volt, 133-cycle transformer.

Through Dr. E. L. Nichols, past president of the Institute, I was invited to present a paper based upon our work on the transformer and the results obtained. The paper was duly prepared and presented at the December, 1889, meeting of the Institute in New York City and was published in the Proceedings in January, 1890."

This paper attracted wide attention, both in this country and abroad, and marked the author out as a pioneer in the effective application of the scientific method to the study of the alternate-current transformer, just then beginning to take an established place in the forefront of electrical industry.

In this investigation by Ryan and Merritt, there was obtained, so far as is known, the first complete record of the instantaneous values of the voltage throughout a complete cycle of an alternating current circuit.

In Ryan's work on alternating current phenomena, following soon after his work on the transformer noted above, much use was made, as a tool of research, of the cathode ray indicator or oscillograph, as it has since come to be called. The cathode ray—an indicator without sensible inertia and susceptible to influence by either a magnetic field or an electrostatic field—was, of course, known since the time of Crookes, and had been used in some forms of scientific research. But, so far as is known, it had not been put to use as a tool in connection with the study of alternating current phenomena. Early in his studies on alternating currents, Ryan adopted the cathode ray as a laboratory tool, developing his own form of apparatus and devising an effective means of magnetic control for bringing the beam to a focus. The application of the cathode ray to the study of alternating current phenomena was, after all, more or less an incident in connection with his wider purposes; but it proved a most valuable agency of research, serving for the exhibition of phenomena which could scarcely have been detected otherwise. It has since, of course, become a common-

place in the investigation of complex periodic electrical phenomena, but to Ryan may be credited its first introduction as an agency in the field of electrical engineering with special reference to the phenomena of alternating currents.

Another outstanding result of Ryan's investigations was given to the public in his paper with M. E. Thompson, "A Method for Preventing Armature Reaction," in the Transactions of the American Institute of Electrical Engineers under date of March 20, 1895. This paper dealt with the results of studies begun in 1892 with his students, on commutation in direct current machines and characteristic behavior in relation to the shape of poles, length of airgap and related factors. The first practical application of the results of these studies was in the Thomson-Ryan generator with pole-face winding, which was the forerunner of the present day interpole type of construction now almost universally used in direct current motors and generators.

But Ryan's chief work, especially in his later years, was focused on problems arising in connection with the long distance transmission of power. In the early nineties of the last century, there were wide differences of opinion regarding the possibilities of transmitting power over long distances and likewise whether direct or alternating current offered the greater promise.

Ryan's first approach to the laboratory study of high voltage phenomena was in 1893 when he constructed at Cornell an oil immersed 30,000 volt transformer. This, on trial, promptly burned out and was replaced by one with air insulation. This was again rebuilt in 1899 for 90,000 volts and continued to give good service for many years thereafter.

It was about this time, in 1897, based on the results of tests made on certain power lines operating in the Rocky Mountains, that serious doubt was cast on the possibility of exceeding 40,000 volts for the long distance transmission of power. These tests indicated that, dependent on a number of obscure factors, electric energy at this voltage would escape rather freely into the atmosphere, thus affecting seriously the efficiency of transmission. The whole future of the long distance transmission of power seemed at stake.

Ryan felt that the conclusions based on these results were too hasty and that the matter called for more extended study. He accordingly undertook a series of investigations continuing until 1904 when he presented a paper entitled "The Conductivity of the Atmosphere at High Voltages". The fundamentals set forth in this paper were a distinct contribution to electrical science. The law of corona formation was established, and the conditions under which corona leakage could be controlled were set forth. The limitation of 40,000 volts was shown to be non-existent, and the way was cleared for the advances in more recent years to a present maximum * of about seven times that earlier apparent limit.

Ryan's studies on insulation and insulators for use on high voltage lines also formed a notable contribution to this phase of power transmission. These were carried on especially over the period 1915-25. These investigations covered the distribution of voltage across the different units making up strings of insulators and the best manner of equalizing the same; the cause and effect of the ageing of porcelain, the causes of failures and flashovers, and allied problems. These studies have been an important factor in those improvements of insulators, both in design and in product, which have made possible the operation of great modern transmission systems with voltages of 220,000 and 275,000.

In connection with his various studies of the problems of high voltage and long distance transmission, Ryan developed a number of new and improved methods of observation and measurement. The adaptation of the cathode ray to the study of periodic phenomena has been mentioned. Another notable example was the high voltage wattmeter. This was a device planned for the more accurate measurement of line losses in high tension power transmission than had been hitherto available. With measurements made on an energized open-circuit line, the results should give, of course, the line losses, or, specifically, the so-called corona loss. And by making such measurements under a series of operating conditions (especially as regards size and character

* The Boulder Dam Power Line Transmission to Los Angeles.

of cable and operating voltage) a relation could be developed between the loss per thousand feet, or per mile of line, and such operating conditions.

The device comprised the necessary current and potential coils, as found in the usual type of low tension wattmeter, the former supplied by a few turns of the main circuit, the latter by a suitable coil and circuit, carrying in series with the coil a special form of water resister. In addition, special shielding arrangements were used in such way that the losses in the leads up to the main line were supplied directly from the transformer and thus eliminated from the indications of the instrument.

By means of this type of wattmeter it became possible to measure with satisfactory accuracy the losses involved in high tension power transmission, and long series of such measurements were made, and related to voltage, type, form and size of conductor, atmospheric conditions, etc., and a vast amount of information obtained of the greatest value in the design of modern high tension transmission lines and their equipment.

In connection with his various studies—on pole face winding, cathode ray magnetic focusing, etc., Ryan took out a number of patents, but he was never one to follow these to his own personal advantage and their benefits went, for the most part, to the industry at large.

In recognition of Ryan's outstanding work in the field of high voltage long distance transmission and as an evidence of the desire for its assured continuance, the leading public service companies of California together with manufacturers of electrical equipment, in 1923-24, provided funds for the establishment of a modern up-to-date high-tension laboratory at Stanford University where fundamental research in connection with these problems could be carried on in advance of the industry. This laboratory, with equipment complete and in working order, was opened in 1926, with demonstration of its capacity for producing and handling electricity under tensions up to 2,000,000 volts. In compliment to the man whose work was recognized by this splendid memorial, the laboratory has been given his name and is known as the Harris J. Ryan High Tension Laboratory.

Ryan continued actively in this laboratory his researches on various phases of long distance power transmission until his retirement from active status in 1931 with passage to the Emeritus role. In further honor, however, he was named Honorary Director of the Laboratory and so continued until his death in 1934.

While Ryan's chief work has lain in the domain of the long distance electrical transmission of power, he made many interesting and effective contacts in other directions. Thus in the domain of radio transmission, it was a student of Ryan's, Mr. C. F. Elwell, who brought the Poulson Arc patents to the United States and in frequent helpful conference with his professor, made the initial experiments which laid the foundation for the Poulson arc radio system as later developed in this country and abroad.

In 1893 Ryan served as a member of the Jury of Awards in the Department of Electricity at the Chicago International Exposition, and again in the same capacity in 1915 at the Panama Pacific Exposition in San Francisco. In 1904 he was a delegate to the International Engineering Congress at St Louis held in connection with the Louisiana Purchase Exposition of that year.

Again, during the war period, Ryan headed a group of researchers at the California Institute of Technology in Pasadena, dealing with the problem of supersonics as applied to the detection of the submarine. This work was carried on under the National Research Council as the agency for the mobilization of the scientific effort of the United States on war problems. The special problem with which this group was concerned was the investigation of ways and means for the increase of energy per unit area of radiating crystal surface, as compared with the values previously obtained. Good progress was being made on the elements of this problem when the Armistice brought the effort to a close.

From 1909 to about 1923 Ryan served as a member of a board of consulting engineers to the Los Angeles Municipal Bureau of Power and Light, especially with reference to the pioneer work of this Bureau in connection with the design of the hydro-

electric generating stations in San Francisquito canyon along the line of the Los Angeles Owen's River Aqueduct. Still later, in 1932-34, important studies relating to the transmission line for power between Boulder Dam and Los Angeles were carried on in the Ryan High Tension Laboratory to which he contributed through frequent consultations regarding the principles involved and the strategy to be employed. As a result, largely of these studies, the practicability of a transmission voltage of 275,000 was clearly indicated for the controlling conditions presented, including in particular, the length of transmission line and the amount of power per circuit. This voltage was adopted for the line, a definite step in advance beyond the then maximum of 220,000, a choice which has been fully justified by the successful operation of this line since the autumn of 1936.

Many opportunities came to Professor Ryan in the way of private consulting practice, but for the most part, they were declined due to physical health and strength none too rugged, and to his absorption in his work of research. He felt that he could not do justice to both and he considered the latter as by far the more important. With the organization and equipment of the High Tension Laboratory, however, and through cooperative arrangements, it became possible to carry on investigations of great fundamental importance, not only to the organization in which the special problem may have arisen, but to the entire profession at large, as witness the investigations relating to the Boulder Dam transmission line and others of similar character.

While Ryan's contributions in the domain of electrical engineering have been of outstanding importance and value, his chief contribution to his day and age is to be found, after all, in the lives and works of the men whom he influenced through the relationship of teacher and student. He viewed his function as a teacher with the utmost seriousness. He was fond of referring to himself as a "Faculty Man" and he considered his work always from the standpoint of helpfulness to his students. For more than forty years one class followed another; in the aggregate a goodly company, taking with them to all parts of the world something gained from their contacts with this man whose first thought always was for the good of his students.

Through this human product the successful teacher achieves an immortality; he lives on in the lives of those whom he has influenced and helped.

Growing out of conditions in what may be termed the new era of Japan, it was but natural that, during the period 1900-30, there should have been a large influx of Japanese students to the United States, seeking instruction in subjects relating to engineering and technology. Especially was this true in electrical engineering, the youngest of the great divisions of engineering activity. Naturally a goodly share of such students gravitated to Stanford University with its convenient location on the Pacific Coast and with opportunities for instruction under Ryan as the immediate goal. Most of these, after graduation, returned to Japan, taking positions either in the industry or in teaching, thus forming a considerable segment of the field of electrical engineering in Japan tracing its training back to Stanford and Ryan, with a few tracing still earlier to Cornell and Ryan.

In addition to students from Japan and from other foreign countries, a number, in the years following the Great War, came to Stanford University on "Commonwealth Fund" and "C.B.R." (Commission for Belgian Relief) fellowships, in order to follow the work in electrical engineering under Professor Ryan.

As evidence of the high regard with which Ryan was held in Japan and especially by his former students both at Stanford and Cornell, he was the first scientist from the United States to be invited to give a course of lectures in Japan on the Iwadare Foundation of the Institute of Electrical Engineers of Japan. This was in 1933. The invitation was accepted, but ill health compelled a cancellation or perhaps rather a postponement of the plans. Later, improving health gave promise that he might be able to make the long anticipated trip to Japan and carry out the program of lectures; but the return to adequate health and strength was denied, and Professor Ryan passed on without having seen his "Carcasonne".

The deep affection felt for Ryan by his former students and the fine sense of personal friendship which they cherished for him, were further shown by an impressive memorial service

held in Tokyo, Japan, on the evening of October 3, following his death.

The gathering was sponsored by Dr. Shibusawa of the Imperial University and attended by twenty electrical engineers and educators in Japanese universities who had studied at Stanford or Cornell and under Ryan.

In a letter to Mrs. Ryan from Mr. S. Motomura, the following paragraph descriptive of this occasion may be quoted:

"After dinner together, the group adjourned to a separate room where a photograph of the late Dr. Ryan was placed in front of a wreath donated by the Stanford Alumni Association of Japan. Reminiscences of their days at Stanford were given by many, in which the greatness of Dr. Ryan as both scientist and man was brought out That the electrical industry in Japan, especially in fields of high voltage, owes a tremendous debt to him was emphasized. A memorial photograph of the group was taken, and after all had silently bowed before the photographic likeness of Dr. Ryan, as a last tribute, the meeting adjourned."

Further evidences of the love and esteem in which Ryan was regarded by his colleagues and former students were shown by a notable dinner given him on the occasion of his retirement from active service in 1931; and following his death in 1934 by resolutions of the Academic Council of Stanford University, of the Governing Body of the American Institute of Electrical Engineers, and by the many letters, telegrams and messages of condolence received by his widow.

The American Institute of Electrical Engineers was organized in 1884. Ryan, as a young graduate of Cornell University, joined in 1887, was advanced to the grade of Member in 1895 and in 1923, on the organization in that Society of the grade of Fellow, he was immediately, with others in a selected group, advanced to that grade. He served the Society on numerous committees, was elected Manager for the period 1893-96, served as Vice-President 1896-98 and as President, 1923-24.

In 1925 he was awarded the Edison Medal of the Society under the citation:

"For his contributions to the science and the art of high-tension transmission of power".

The ceremonies of award were held on the occasion of the Pacific Coast Convention of the Institute in Salt Lake City on September 8, 1926. Ryan's life work was reviewed by Mr. Paul M. Downing, Vice President of the Society, and the award was made by President C. C. Chesney. In his reply, Ryan made references to some of his earlier work in terms from which quotations have been given at earlier points.

Besides the American Institute of Electrical Engineers, Ryan was a Fellow of the American Association for the Advancement of Science, and held membership in the American Society of Mechanical Engineers, the American Electro Chemical Society, the Institute of Radio Engineers, the American Physical Society and the Society for the Promotion of Engineering Education. He was also a member of Sigma Xi and of Tau Beta Pi. In 1920 he was elected to the National Academy of Sciences. In matters of technical society activity, however, his chief loyalty was always with the American Institute of Electrical Engineers, and practically all of his papers dealing with original investigations were given to the public through the Transactions of that body.

On graduating from Cornell, Ryan received the degree of M.E., (E.E.), one of the earliest to receive a distinctive degree in electrical engineering from an institution of higher learning. In 1925 he was awarded the honorary degree of LL.D. by the University of California.

In approaching any general integration of Professor Ryan's outlook on life or his attitude toward his professional work, note must first be taken of his friendliness with all with whom he came in contact, and of his capacity for making and holding friends. This was evidenced not only by the continuing warm friendly relations maintained with those who had come to know him as a teacher, but by all with whom he came in contact in the various relations of life. There was always a pleasant greeting, a kindly word and a friendly interest in the problems and troubles of others.

In his scientific and technical work, he was always thinking of the future. His problems and his chief interests were centered on the long look ahead. His was distinctly the spirit of

the pioneer as witness his early work on pole face windings and on the transformer, on the electrical properties of the atmosphere with reference to the high tension transmission of power, his studies on insulation and insulator chains and other like problems. Much of the development of the applied art in electrical engineering followed upon and was conditioned by the results of these studies.

On September 12, 1888, the young Instructor Ryan married Katherine E. Fortenbaugh, a girl of his home town, Halifax, Pa. Mrs. Ryan joined most heartily with her husband in making their home a place of cheerful welcome for students, and the Ryan house was always a shifting scene of friendly calls from students past and present, with always a warm greeting and kindly interest in their welfare. Mrs. Ryan must share with Professor Ryan in the making of this atmosphere of friendly welcome which was always so characteristic of the Ryan home.

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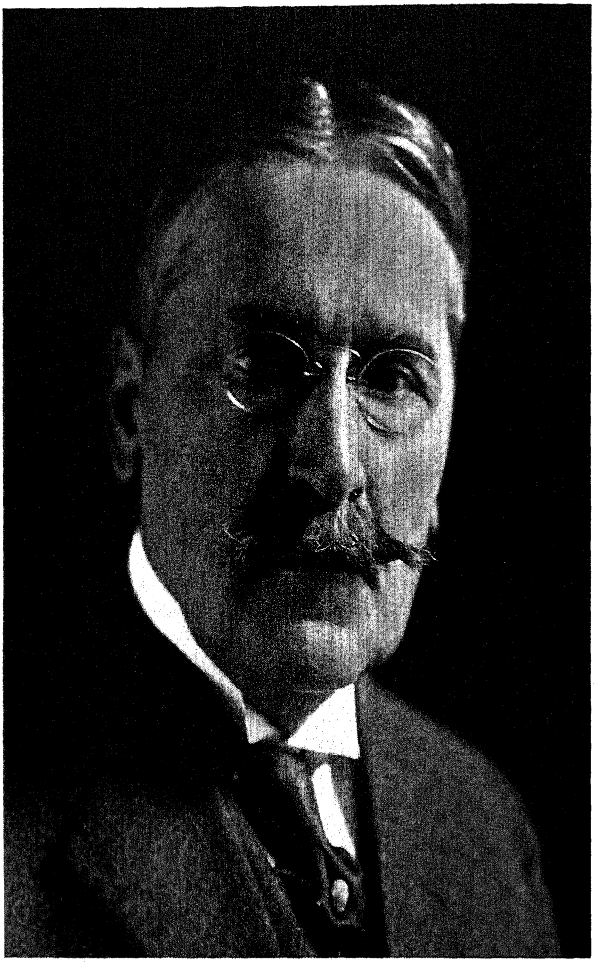

M. I. Pupin

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OF

MICHAEL IDVORSKY PUPIN

1858–1935

BY

BERGEN DAVIS

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MICHAEL IDVORSKY PUPIN

1858–1935

BY BERGEN DAVIS

The subject of this memoir was born on October 4, 1858, in the small village of Idvor in the Banat of Austria—an Austrian citizen, he was pure Serbian in race. His father, Constantine, and his mother, Olympiada, were sturdy peasants in moderate circumstances. The parents illustrate the very significant difference between intelligence and education. Neither of them could read or write. His father, however, was several times elected "knez" or chief of his village. His mother was a woman of great piety, wisdom and mental vigor. All readers of "From Immigrant to Inventor" will be impressed by the great influence she exerted on the character, early education, and future career of her son Michael.

In 1690, upon the invitation of King Leopold I of Austria, thirty-five thousand picked Serbian families moved from Serbia to the north side of the Danube River into a province known as the Banat. These Serbs were invited there to guard the Austrian frontier against the Turks. A narrow strip of territory was assigned to them. Idvor was one of these settlements. Although technically citizens of Austria, these settlers retained the language and folk-ways of old Serbia.

From this ancestry and from the struggle with the physical characteristics of the land, Pupin acquired great powers of self-discipline, and also derived his poetical temperament, his imagination and his courageous spirit.

His formal education was begun in the village school of Idvor, where he learned reading, writing and arithmetic. As he showed marked mental vigor and capacity, his mother prevailed upon his father to send him to school first in Pechavo, and later to Prague. An incident that occurred on his journey to Prague had an influence on his later emigration to America. He journeyed down the Danube to Budapest, where he took a train bound for Vienna. He was to have changed at Gaenserndorf for Prague. The compartment was warm. He fell asleep and was

carried directly into Vienna. Having no money to pay his return fare, he was hailed before the station master, in the boy's eyes, an awesome official in uniform. He was brow-beaten and insulted by this petty official. An elderly couple who witnessed the affair came forward and offered to pay the young man's fare to Prague, to which place they themselves were bound. They took him with them into a first-class coach. On this journey, he learned that his new-found friends were from America, the country of Benjamin Franklin of whom he had read. During this ride from Vienna to Prague, these generous Americans treated him as an equal and, on arrival in Prague, took him to their hotel as a guest until he could get established in lodgings.

After about a year in Prague, his father died, and, fearing his schooling would be too great a burden on his mother, he decided to emigrate to America. To get the necessary money for his passage to New York in the steerage, he sold his books, his watch, and his great sheepskin coat. He sailed for New York on March 12, 1874, on the steamship "Westphalia." He suffered greatly from cold on this trip (minus his great coat). His experiences form an interesting paragraph in his autobiography. After a painful voyage of fourteen days, he landed in New York on March 26th with five cents in his pocket and a red fez on his head.

His first job was that of driving a team of mules on a farm in Delaware. He also began to learn English under the tutelage of the daughter of the farmer. He also learned much of American thought and customs, which were new and strange to him. After a few months on this farm, he drifted back to New York City and spent the winter of 1874-75 in great hardship. He walked the streets seeking work, performing odd jobs of painting and unloading coal into basements. The evenings were spent in reading in the library of Cooper Union. In the spring of 1875 he again went to work on a farm near Dayton, New Jersey. The persistent efforts of the farmer and family to convert him to their narrow religious views became intolerable. He returned to New York and finally found work in a cracker factory on Cortlandt Street. His experiences there and the accounts of his friends Jim and Bilharz form one of the most

interesting chapters in "From Immigrant to Inventor." The early chapters of this book are of great interest and should be read by all who are interested in the problem of Americanization of immigrants. During the next three years, he attended evening classes at Cooper Union and strenuously trained himself in English by conversation, discussion with Bilharz and Jim, and by attendance at the theater. In the meantime, by industry and thrift he accumulated a modest sum in a savings bank. His friend Bilharz at the cracker factory, who was a classical scholar, helped him to prepare for college.

His career at Columbia College, which he entered in the autumn of 1879, was a successful one, not only academically, but also in a social way. He took a prominent part in athletics, won a number of prizes in Greek and mathematics, and was elected president of his class in his senior year. While in college, he became much interested in the classics, particularly Greek, but his enthusiasm for physical science was aroused by witnessing Faraday's experiment on electro-magnetic induction as performed by Professor Rood in the classroom. He resolved to find out all about this wonderful phenomenon. On graduation in 1883, he was appointed first Tyndall Fellow in Physics from Columbia College. The next six years were spent at the Universities of Cambridge, England, and Berlin.

At Cambridge he was occupied principally with mathematical physics and became especially interested in the electro-magnetic theory of Maxwell. After a year or so at Cambridge, he went to Berlin and, at the suggestion of Helmholtz, began a research in the new science of physical chemistry. He completed a dissertation on the subject of "Osmotic Pressure and Free Energy," receiving the degree of Doctor of Philosophy in 1889.

Before returning to America, Professor Pupin married Sarah Katherine Jackson, the widow of Frederick J. Agate, and the sister of his classmate and future colleague, Professor A. V. Williams Jackson.

Pupin returned to Columbia as instructor in the newly created department of electrical engineering, he and his friend, Francis B. Crocker, constituting the whole staff of that department. Later he was made Adjunct Professor, and in 1901 was ap-

pointed Professor of Electro-Mechanics. The basic theoretical courses in electrical engineering were given by Pupin in the mornings, and in the afternoons he was required to give laboratory instruction. Notwithstanding a heavy teaching load, he found time in the evenings to carry on with experimental research. His earliest work was in the field of electrical phenomena associated with the discharge of electricity through gases.

As a result of his teaching duties, he became interested in electrical engineering problems and occupied himself with the experimental investigation of the peculiarities of wave forms of alternating currents, to which Professor Rowland of Johns Hopkins University had called his attention. His familiarity with the methods of Helmholtz in analyzing complex sound wave forms by means of resonators enabled him to apply a similar method to the analysis of complex current wave forms. This he accomplished by the use in electrical circuits of adjustable condensers and inductances. He was thus led to the discovery of electrical tuning, which is so essential in the art of radio communication.

In December, 1895, Roentgen announced his epoch-making discovery of X-rays. Pupin became interested immediately, and two weeks later, on January 2, 1896, he took the first X-ray photograph made in America. With the aid of a phosphorescent screen, furnished by his friend Thomas A. Edison, superimposed on a photographic plate, he was able to make good X-ray pictures in a few seconds of exposure. In a communication to the New York Academy of Sciences on April 6, 1896, he announced the discovery of secondary X-radiation and is now generally accorded priority for that discovery.

On April 15, 1896, Professor Pupin was stricken suddenly with pneumonia. After a few days of anxious care of him, Mrs. Pupin was also seized with the same disease and died after a short illness. This illness and loss were a great shock, and were followed by great depression of spirit. Upon the advice of his physician, he spent the following summer at Norfolk, a village in the Berkshires, in the northwest corner of Connecticut. Convalescence was slow until his physician, a lover of horses, presented him with a pair of beautiful young cobs only partially

trained from his stud. Boyhood love of horses and his interest in training them brought back health by the end of the summer. Comet and Princess Rose, under his skillful training, became prize winners at the New York and Philadelphia horse shows of 1897-98.

Naturally, he became much attached to Norfolk and the following year purchased a farm near the village. About ten years later, using stones from the fields, he built his picturesque summer home, to which he was accustomed to go not only for rest, but also for opportunity to work, free from the distractions of city life. This home is now the residence of his daughter, Mrs. L. Graham Smith, and her husband.

Upon recovery from his illness, Pupin returned to a problem which eventually led to the invention of the "loaded line." While in Paris, fifteen years previously, he had found at a riverside bookstall a treatise by La Grange containing a paper, "Researches sur la Nature et la Propagation du Son," in which a solution was given of the problem of a vibrating string fixed at each end and loaded at equal intervals with equal masses. Pupin now proposed to generalize the conditions assumed by La Grange, through assuming the string itself to have mass, and the medium surrounding the string to exert a dissipative reaction to its motion. He was able to obtain a solution of this more general problem, but did not at the time realize its tremendous practical importance.

This investigation furnished a solution of the exactly analogous problem of the propagation of electro-magnetic signals over telephone lines having distributed capacity and loaded at equal intervals with inductances. Professor Pupin's investigation showed that the malevolent influences of capacity and resistance in causing distortion and attenuation could be overcome by the introduction of inductances at specified distances along the line.

This invention of the "loaded line" was of the highest importance in telephone transmission. Before this invention, long distance telephoning was impossible. Also, before the year 1900, the streets of large cities such as New York were disfigured by telephone poles carrying hundreds of wires. It was not prac-

tical to transmit, even for short distance, over wires placed underground. The loaded line made transmission possible on underground cables over much longer distances. The unsightly overhead wires completely disappeared from our city streets.

The American rights to this invention were acquired by the American Telephone and Telegraph Company. This "loading" is used universally in the art of telephone transmission throughout the world. The German rights were acquired by the Siemens and Halske Company. This loading of telephone lines is called "pupinization" on the continent of Europe.

The use of loading coils had an enormous growth in the first quarter-century after this invention. By 1911 there were 125,000 loading coils in use on 85,000 miles of open circuit and 170,000 miles of cable circuits. By 1926 there were 1,250,000 coils in use on 1,600,000 miles of cable and 250,000 miles of open wire circuits. The writer is informed by an engineer of the American Telephone and Telegraph Company that at the end of 1936 there were over 8,500,000 loading coils in service in the United States. There are approximately 5,000,000 circuit miles of loaded toll cable and 4,000,000 circuit miles of loaded local cable in use.

A prominent electrical engineer has estimated that in the first twenty-five years this invention saved the American people more than one hundred million dollars.

Not long after the sale of his telephone invention, Pupin disposed also of his wireless inventions, such as electrical tuning and the electrolytic rectifier, to the Marconi Company of America. For many years after the first announcement of the principle of "loading," he was immersed in the many problems that arose in connection with the practical application of loading coils to telephone circuits. These practical problems absorbed much of his attention up to the outbreak of the World War. These problems and this latter event diverted Pupin's attention and prevented his participation in the advancement of pure physics in which he began his career and in which he always remained so much interested.

After the solution of the many problems that arose in connection with the introduction of the "loaded line" into prac-

tice, Pupin carried forward many interesting experiments in alternating current phenomena. As early as 1899 he developed and published a theory of artificial lines called "net works." This theory contained the mathematical foundations used in the construction of electrical filters at the present time. The negative resistance idea was suggested by Pupin and was first produced by running an induction motor beyond synchronism. He showed that if such a negative resistance is introduced in a circuit containing inductance, capacity and resistance, continuous electrical oscillations can be produced. A student, E. H. Armstrong, working in his laboratory, produced this negative resistance by means of the three electrode vacuum tube. This led him (Armstrong) to the invention of the high frequency vacuum tube oscillator, which is the foundation of modern radio broadcasting.

In his later years Pupin withdrew largely from personal scientific research. His time and thought, especially after the World War, were given to semi-public affairs. He made many public addresses before scientific and educational institutions. The underlying theme of many of these addresses was idealism in science and American idealism in life. He was an eloquent speaker, possessed a forceful personality and was endowed with a poetic imagination that gave great pleasure and profit to popular audiences.

One result of this popular activity was the writing and publishing of that remarkable book, "From Immigrant to Inventor." In this he tells of his boyhood life in Idvor, of his journey to America at the age of sixteen, of his hardships and fortitude in these, his struggling years. A book remarkable for its merit as literature, it is filled with the charm of a poetic imagination. Here, too, a major theme is idealism in American science. This book had a great popular appeal and has had a large and continuous sale up to the present time. It has been translated into several foreign languages, and letters of appreciation from all over the world gave the author assurance that his time and labor had not been in vain. On account of persistent demand, an abridged edition was issued suitable for use in the public schools.

In 1927 he issued another book based on the ideas he had put forth in his popular addresses. The title was "The New Reformation." The purpose of this book may be inferred from the concluding words of the prologue: "It is hoped by strengthening our understanding of the physical realities, these narratives will reform our mental attitude and make it better prepared for the recognition of the truth that physical and spiritual realities are the fruit of the same tree of knowledge, which was nurtured by the soil of human experience." In this book, writes his colleague and friend, Professor A. P. Wills, "Pupin has revealed the simple and rational philosophy of life to which he adhered and the spiritual sentiments which were a part of his religious faith."

At the outbreak of the Balkan War in 1912, Pupin was appointed Honorary Consul General and, I believe, was the only diplomatic representative of Serbia in America during the Balkan and the World Wars. He started at this time, at his own expense, a Serbian daily newspaper, mainly for the purpose of keeping Serbian immigrants informed as to the war movements in the Balkans. He also organized a Serbian sisterhood whose members were encouraged to collect contributions to the Serbian Red Cross, and, in the interest of the Serbian National Defense League, to inspire volunteers for the service. In 1914 this work was extended throughout the United States, and highly satisfactory results were obtained.

As the only representative of Serbia in America during the World War, he had charge of that government's purchases, and in at least one instance personally assumed large financial responsibility for the same. He headed the committee formed in this country to help Serbian war sufferers. He was active in the formation of the Serbian Child Welfare Association which did noble work in providing medical supplies, clothing and homes for Serbian war orphans.

At the conclusion of the World War, Premier Paskitch invited Pupin to serve as Serbian representative at the Paris peace conference in April, 1919. Here, in collaboration with his colleague, Professor Douglas Johnson, he was able to advance arguments which resulted in extending materially the proposed

boundaries of the newly created Kingdom of the Serbs, Croats and Slovenes, now known as Yugoslavia.

On the entrance of the United States into the World War in 1917, Professor Pupin organized a group at Columbia University for research into methods of detecting submarines. Together with his colleagues, Professors A. P. Wills and J. H. Morecroft, he carried forward many interesting and valuable experiments on submarine detection at Key West, Florida, and New London, Connecticut, making use of supersonic waves. During the war he served as member of the National Research Council and the National Advisory Committee for Aeronautics. After the war he helped munificently to restore the Serbian churches, schools, museums, etc., for which cause he contributed about one hundred thousand dollars over a period of fourteen years.

In 1911 he had established a memorial fund of $25,000 with the Royal Society in Belgrade in memory of his mother, Olympiada Pupin, the income of the fund to be used for scholarships. In 1928 he established a trust fund of ten million dinars (about $250,000). This fund, which is at the disposal of the Serbian Cultural Society of Belgrade, is to be used for scholarships. Just before his death, he established another fund, the Pupin Memorial Fund of two million dinars (about $50,000). Out of this fund a Pupin memorial home was built in his native village of Idvor. In this there are classrooms for students in agriculture, lecture rooms, and a large hall for moving pictures, concerts, etc. The fund provides several annual scholarships for boys of Idvor for advanced study of agriculture. He also gave other sums for cultural purposes to the National Museum in Belgrade and the Museum of Art in Zagreb.

A few years before his death Professor Pupin transferred the remainder of his property to Columbia University, subject, however, to certain life interests. At the expiration of these life interests, the income is to be applied to the support of research in physics and physical chemistry.

For many years Pupin had been in demand as a speaker on public occasions. This demand increased greatly after the publi-

cation of "From Immigrant to Inventor." He was always an interesting and impressive speaker to the layman in science. He did much service in arousing popular interest in science, and especially in combating that strange notion that prevails in some quarters that over-development of science is a potent cause of our economic ills.

In person, Professor Pupin was a large, vigorous man who gave the impression of great reserve of physical strength. He had a vivid personality that impressed all who came in contact with him. He was fond of social life, was of fine breeding and possessed the social graces to a high degree. He was generous and hospitable in social intercourse, and for the last thirty-five years of his life it was his custom to invite his many friends in succession to enjoy visits during the summer at his home in Norfolk.

Pupin's great physical vigor lasted him up to seventy years of his life, when a decline in health began. A partial paralysis of the legs set in which gradually increased, and finally he was not able to walk, but was confined to chair and bed. He died at the Harkness Pavilion at the Medical Center, New York City, on the twelfth day of March, 1935.

Below this sketch is appended a list of his many honors, awards and degrees. A list is also given of his published papers and of his patents.

Professor Pupin received many honors and awards for his services to engineering, to science, and to public affairs.

Awards:

The Elliot Cresson Medal of the Franklin Institute.
The Edison Medal of the American Institute of Electrical Engineers.
Honor Medal of the Radio Institute of America.
Honor Medal of the Institute of Social Sciences.
The Herbert Prix of the French Academy.
The John Fritz Medal of the Four National Engineering Societies.

Awards and Decorations:

George Washington Award of the Western Society of Engineers. 1928.
White Eagle, First Order of Yugoslavia. 1929.
White Lion, First Order of Czechoslovakia. 1929.

Membership in Societies:

National Academy of Sciences.
American Mathematical Society.
American Philosophical Society.
American Physical Society.
Honorary Member, American Institute of Electrical Engineers.
Honorary Member, German Electrical Society.
Corresponding Member, Royal Serbian Academy, Belgrade.

President of:

New York Academy of Sciences.
Radio Institute of America.
American Institute of Electrical Engineers.
American Association for the Advancement of Science.
University Club of New York,
also, Chairman of the Engineering Foundation.

Honorary Degrees:

Year	*Degree*	*Institution*
1904	Sc.D.	Columbia University
1915	LL.D.	Johns Hopkins University
1924	Sc.D.	Princeton University
1924	LL.D.	New York University
1924	LL.B.	Muhlenberg College
1925	D.Eng.	Case School of Applied Science
1925	L.H.D.	George Washington University
1925	Sc.D.	Union College
1926	LL.D.	Marietta College
1926	LL.D.	University of California
1926	Sc.D.	Rutgers University
1926	LL.D.	Delaware University
1926	LL.D.	Kenyon College
1927	Sc.D.	Brown University
1927	Sc.D.	Rochester University
1928	LL.D.	Middlebury College
1929	Sc.D.	University of Belgrade, Yugoslavia
1929	Sc.D.	University of Prague, Czechoslovakia

BOOKS BY MICHAEL I. PUPIN

Thermodynamics of Reversible Cycles in Gases and Saturated Vapors. John Wiley & Sons. 1894.

Serbian Orthodox Church, edited by Michael I. Pupin. . . . with an introduction by Sir Thomas Graham Jackson, bart. London, J. Murray. 1918. 64 pp., 64 pl.

Yugoslavia. (In Association for International Conciliation Amer. branch —Yugoslavia). American Association for International Conciliation. 1919.

From Immigrant to Inventor. New York, Scribner. 1923. 396 pp.

The New Reformation; from Physical to Spiritual Realities. New York, Scribner. 1927. 273 pp.

Romance of the Machine. New York, Scribner. 1930. 111 pp.

Discussion by M. Pupin and other prominent engineers in "Toward Civilization," edited by C. A. Beard. New York, Longmans, Green & Co. 1930.

SCIENTIFIC PUBLICATIONS OF M. I. PUPIN

1889

Der Osmotische Druch und seine Beziehung zur Freien Energie. Inaugural Dissertation, Berlin, June, 1889.

1890

Practical Aspects of the Alternating Current Theory, May 21, 1890. Trans. Amer. Inst. Elec. Eng., Vol. vii, 204, June and July, 1890.

1891

On Polyphasal Generators, Dec. 16, 1891. Trans. Amer. Inst. Elec. Eng., Vol. viii, Dec., 1891.

The Characteristic Features of the Frankfurt Electrical Exhibition. School of Mines Quarterly, Nov., 1891.

1892

On the Action of Vacuum Discharge Streamers upon Each Other. Amer. Jour. Sci., April, 1892.

On Electrical Discharges through Poor Vacua and on Coronoidal Discharges. Amer. Jour. Sci., June, 1892.

1893

New Method of Measuring the Solar Corona without an Eclipse. Astron. & Astro Phys., April, 1893.

On Electrical Oscillations of Low Frequency and their Resonance. Amer. Jour. Sci , April, May and June, 1893.

Practical Aspects of Low Frequency Electrical Resonance, May 17, 1893. Trans. Amer. Inst. Elec. Eng., Vol. x, 370, June and July, 1893.

1894

Resonance Analysis of Alternating and Polyphase Currents, May 17, 1894. Amer. Jour. Sci., Nov., 1894. Trans. Amer. Inst. Elec. Eng., Vol. xi, Oct., 1894.

Submarine Rapid Telegraphy and Telephony. Elec. World, May 19, 1894.

System of Resonating Conductors for Telegraphy and Telephony. Elec. Eng., May, 1894.

1895

An Automatic Mercury Vapor Pump. Amer. Jour. Sci., January, 1895.

The Most General Relation Between Electric and Magnetic Force and their Respective Displacements. A.A.A.S. Proc., 1895; 55-56.

Electrical Consonance. Elec. World, Feb. 9, 1895.

"Les Oscillations Electriques," by H. Poincare (Review), Science, Jan. and Feb., 1895.

Studies in the Electro-Magnetic Theory. I. The Law of Electro-Magnetic Flux. Amer. Jour. Sci., Series 4, Vol. 1, 1895.

Tendencies of Modern Electrical Research. Address delivered before the New York Academy of Sciences, April 28, 1895. Science, Vol. ii, No. 52, Dec., 1895.

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Roentgen Rays. Science, 1896.

Diffuse Reflection of Roentgen Rays (Science, April, 1896), announcing Pupin's discovery of secondary X-ray radiation.

1899

Propagation of Long Electrical Waves, Mar. 22, 1899. Trans. Amer. Inst. Elec. Eng., Vol. xv, 144.

Magnetizing Iron with Alternating Currents, Preliminary Account. (1899.) N. Y. Acad. Sc. Annals 1899-1900, 658.

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Wave Transmission over Non-Uniform Cables and Long-Distance Airlines. Trans. Amer. Inst. Elec. Eng., Vol. xvii, May 18, 1900.

Wave Propagation over Non-Uniform Electrical Conductors. Trans. Amer. Math. Soc., July, 1900.

Electrolytic Rectifier of Alternating Currents. Bull. Amer. Phys. Soc., Vol. i, 20.

A Faradmeter. Trans. Amer. Inst. Elec. Eng., Vol. xvii, 75-77, 1900.

Wave Propagation Over Bridged Wave Conductors. (1900). Amer. Math. Soc. Bul., 1901; 7, 202, 205-206.

1901

Transatlantic Communication by Means of the Telephone. Eng. Mag., Vol. 21, 105-14. April, 1901.

A Note on Loaded Conductors. Elec. World and Eng., Vol. 38, 587-588, Oct. 12, 1901.

1902

The General Problem of Wave Propagation over Non-Uniform Conductors. Elec. World and Eng., Mar. 1, 1902.

1907

Distortion in Telephone Transmission. Elec. World, Vol. 50, 927, Nov. 9, 1907. (Letters to the editors.)

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Aerial Transmission Problems. Science, Dec. 10, 1915. n.s. 42, 809.

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1933

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1934

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1894

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1901

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A Herdsman's View of Human Life. Colum. Alum. News, Jan. 13, 1922; Vol. XIII, 197-199.

An Immigrant Student at Columbia in 1879. Colum. Alum. News, Dec. 15, 1922; Vol. XIV, 137-140.

1923

America's Position in Physical Sciences. Colum. Alum. News, Feb. 9, 1923; Vol. XIV, 235-237.

The Spiritual Influence of a Noted Scientist. (Henry Marion Howe.) Colum. Alum. News, Nov. 2, 1923; Vol. XV, 66-67.

1924

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From Chaos to Cosmos. Scribner's Magazine. Vol. 76, 3-10, July, 1924.

1925

The Meaning of Scientific Research. Science, n.s. Vol. 62, 26-30. Jan. 9, 1925.

Law, Description and Hypothesis in the Electrical Science. Science, n. s. Vol. 62, 17-22. July 1, 1925.

Chandler: the Teacher and the Chemist. Science, n.s. Vol. 62, 499-501. Dec. 4, 1925.

1926

Lincoln's Revelation to a Serbian Immigrant. (Address delivered at Springfield, Illinois, Feb. 12, 1926.) Bull., Lincoln Memorial Association.

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The New Reformation: the Triumph of Individualism in Science. 1926. Scribner's Magazine, Vol. 79, 113-20, 275-83, Feb.-Mar., 1926.

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Cosmic Harness of Moving Electricity. (Presidential address, American Institute of Electrical Engineering, White Sulphur Springs, 1926.) Journ. Amer. Inst. Elec. Eng., Vol. 45, 758-61. August, 1926.

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Higher Endeavor in Science. Science, n.s. Vol. 65, 509-11. May 27, 1927.

Creative Coordination; a Message from Physical Science. Scribner's Magazine, Vol. 82, 142-53. August, 1927.

Creative Coordination. School and Society, Vol. 26, 543-7, October 29, 1927.

1928

Our Industrialism and Idealism. Scribner's Magazine, Vol. 83, 659-664. June, 1928.

Unity of Knowledge. Methodist Review, Vol 111, 169-75. March, 1928.

1929

The Pioneering Professors. Scribner's Magazine, Vol. 85, 123-6. Feb., 1929.

1930

Romance of the Machine. Scribner's Magazine, Vol. 87, 130-7. Feb., 1930.

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Man and the Universe. Review of Reviews, Vol. 83, 73-85, March, 1931.

Introductory remarks (at a dinner given in honor of Sir James Hopwood Jeans in New York). Scientific Monthly, Vol. 33, 10-11. July, 1931.

1933

Our Debt to Joseph Henry, Scientist. American Scholar, Vol. 2, 132-8. March, 1933.

A Message from Science. Scribner's Magazine, Vol. 93, 300-3. May, 1933.

LIST OF PATENTS

1894

519,346 Apparatus for telegraphic or telephonic transmission
519,347 Transformer for telegraphic, telephonic, or other electrical systems

1900

640,515 Distributing electrical energy by alternating currents
640,516 Electrical transmission by resonance-circuits
652,230 Reducing attenuation of electrical waves and apparatus thereof
652,231 Reducing attenuation of electrical waves

1902

697,660 Winding-machine
707,007 Multiple telegraphy
707,008 Multiple telegraphy
713,044 Producing asymmetrical currents from symmetrical alternating electromotive forces
713,045 Apparatus for producing asymmetrical currents from symmetrical alternating electromotive forces

1904

761,995	Apparatus for reducing alternation of electrical waves
768,301	Wireless electrical signalling

1906

821,741	Telegraphy

1920

1,334,165	Electric-wave transmission
1,336,378	Antenna with distributed positive resistance

1921

1,388,441	Multiple antenna for electrical wave transmission
1,399,877	Sound generator

1922

1,415,845	Selectively opposing impedance to received electrical oscillations
1,416,061	Radio-receiving system having high selectivity

1923

1,446,769	Aperiodic pilot conductor
1,456,909	Wave conductor
1,452,833	Selective amplifying apparatus

1924

1,488,514	Selective amplifying apparatus
1,494,803	Electrical tuning
1,502,875	Tone-producing radio receiver

1925

1,541,845	Electrical wave transmission
1,561,278	Wave signalling system
1,561,279	Equalizing vacuum-tube amplifier

1926

1,571,488	Electromagnetic production of direct current without fluctuations

1928

1,657,587	Electrical pulse generator

1931

1,834,735	Inductive artificial line
1,811,368	Telegraph system

1934

1,983,774	Supply system for vacuum tubes

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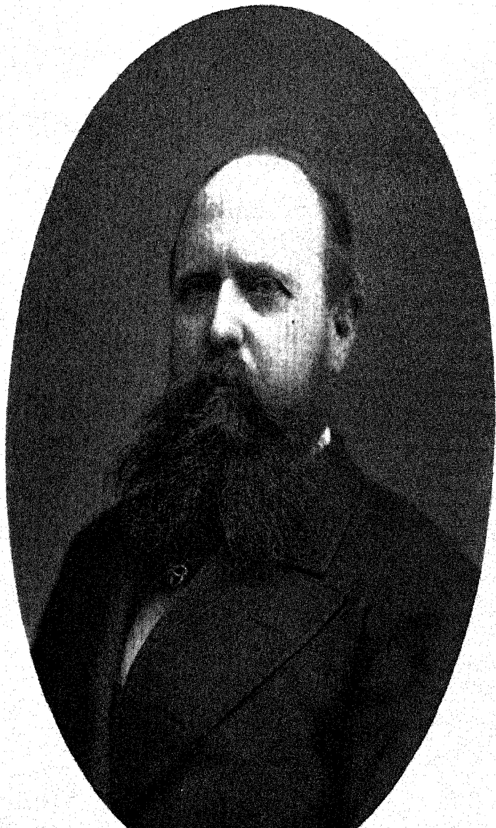
CONTENTS



O C Marsh

NATIONAL ACADEMY OF SCIENCES
OF THE UNITED STATES OF AMERICA
BIOGRAPHICAL MEMOIRS
VOLUME XX—FIRST MEMOIR

BIOGRAPHICAL MEMOIR

OF

OTHNIEL CHARLES MARSH

1831–1899

BY

CHARLES SCHUCHERT

PRESENTED TO THE ACADEMY AT THE ANNUAL MEETING, 1938

OTHNIEL CHARLES MARSH[1]

1831–1899

BY CHARLES SCHUCHERT

Othniel Charles Marsh, for twelve years president of the National Academy of Sciences, was born to Caleb Marsh and Mary Gaines Peabody on October 29, 1831, in Lockport, New York, and died in New Haven, Connecticut, on March 18, 1899. One of the three founders of the science of vertebrate paleontology in America, his career furnishes an outstanding example of the indomitable spirit that drives men on to a determined goal. His motto might well have been, "What I have, I hold." He asked no quarter, and gave none. At home around a camp fire or in an army tent, formal as a presiding officer or in society, at times austere and autocratic, at others a raconteur of note, he left a lasting impression on his chosen branch of science.

Summarizing his work statistically, it may be said that between 1861 and 1899 he published about 300 papers, reports, and books. Of new genera he described 225, and of new species, 496; of new families 64, of suborders 8, of orders 19, and of subclasses 1.

Of his work on vertebrate fossils in general, Osborn says that he "carried out the most intensive field exploration known to science and published a large number of preliminary papers, which fairly revolutionized our knowledge."

ANCESTRY AND TRAINING

John Marsh of Salem, the first of his name recorded as emigrating from England to America, is believed to have reached

[1] In the preparation of this memorial, the writer has been aided greatly by the excellent sketches of Professor Marsh written by George Bird Grinnell, Charles E. Beecher, and J. L. Wortman. Still further insight into his background and character was furnished by the many family letters preserved in the archives of the Peabody Museum at Yale University, and by the twenty-six bound volumes of Marsh correspondence. The memorial has also had the benefit of criticism by Professor Richard S. Lull, who was for many years in charge of the Marsh collections at Yale. It is the writer's hope soon to expand the memorial into a book on Professor Marsh.

the Massachusetts Bay colony in the year 1634. A cordwainer by trade, he was very fortunate in his marriage, taking to wife in 1635/36 Susanna, eldest daughter of the Rev. Samuel Skelton, the latter a graduate of Cambridge and the "spiritual father" of Governor Endicott, at whose solicitation he came to Salem in 1629 and there organized the first church of the Puritans. Zachary, the eldest of John and Susanna Marsh's eleven children, in turn left a family of nine, and it is this line that has most interest in the present connection, because John Marsh, of the sixth generation from Zachary, and his wife, Mary Brown, were the parents of Caleb Marsh, born in South Danvers (now Peabody) on November 8, 1800.

On his mother's side, Professor Marsh's ancestry can be carried back much further, the name Peabody (or Pabody) reputedly originating early in the Christian era. Lieut. Francis Peabody, the first of the American family, came to New England from Hertfordshire in 1635—a "husbandman," 21 years of age. He lived first in Ipswich but in 1638 became one of the original settlers of Hampton, and moved to Topsfield in 1650. Like John Marsh, he picked a helpmate from a distinguished family, the daughter of Reginald Foster (or Forster), whose kin were honorably mentioned in "The Lay of the Last Minstrel" and in "Marmion". Thomas, their descendant of the fifth generation, born in 1762, married Judith Dodge of Rowley in 1788, and became the father of Mary Gaines Peabody, Professor Marsh's mother, on September 3, 1807.

It is interesting to note the purity of the English stock that lay back of the subject of this memorial. The families that are represented by Professor Marsh's eight greatgrandparents can each be traced back to the early days of the colonies without break, and several of them are known for many generations in England; and in none of the marriages so far traced does there appear any name indicative of other than English blood. The families were: Marsh, Brown, Foster, and Buxton, on the one side, and Peabody, Gaines, Dodge, and Spofford on the other.

The early years of the nineteenth century found the two households with which we are concerned living in the village of South Danvers. John and Mary Marsh, with their seven children.

shared a comfortable home, kept from ready money by the father's tendency to acquire still more land, but with definite educational traditions that sent the boys to nearby academies and at least two of them to college—John [2] to Harvard, Ezekiel to Bowdoin and then to Andover and Yale for theology. In the Peabody home, there was at first little surplus beyond living necessities, because the father had died by accident in 1811, leaving his widow to find food and shelter for eight children, the youngest of whom had been in the world only two years. Judith Dodge Peabody was made of stern stuff, however, and she had stalwart help in her sons, especially George, then aged sixteen, who was to have one of the most astonishing careers in American finance. By the time the younger girls were ready for schooling, George was providing the family with a comfortable living, and it was at his request that his sisters Mary and Sophronia were sent to Bradford Academy, which Caleb Marsh had also attended.

The Peabody family moved from Danvers to a neighboring town some time during this period, but Mary's friendship with Caleb Marsh reached the stage of an engagement in the early part of 1826. A letter was sent to Baltimore, asking her brother's consent to the marriage, and enclosing an extract of a letter from Caleb, which was concerned with the very necessary question of what the young couple were to live on. George Peabody agreed to give his sister as much as Caleb should receive from his father, so the two seemed assured of a comfortable amount on which to start life together.

Caleb and his family felt that his opportunities for making a living would be better in the new country to the west, and with that idea in mind the young man accompanied his brother John to Michigan. However, the farm that Caleb finally bought for his matrimonial venture was in Lockport, New York, on Chestnut Ridge, three miles east of the village and one mile from the Erie Canal, on the north side of the old Post Road to Albany; it had 114 acres of land, two "convenient houses", three large

[2] The Dr. John Marsh whose interesting story has been told by George D. Lyman in his book, *John Marsh, Pioneer,* Scribner's, 1930.

barns, and a very fine orchard, the price being $2000. Later he bought an additional 100 acres or more.

To this Lockport farm Caleb brought his young wife shortly after their marriage on April 12, 1827. No sorrow seems to have clouded their first years together except the loss of their first child at birth early in 1828, a loss soon softened by the coming of a daughter, Mary, in August 1829, and of a son, Othniel Charles, on October 29, 1831. There were temporary reverses of fortune such as fall to the farmer's lot, but no major catastrophe until August 1834, when the young mother, apparently recovering normally from the birth of her fourth child, was taken down with cholera and died within fourteen hours. Her husband, grief stricken, hurriedly sold his farm and went back with his two elder children to his old home in Danvers. In 1836 or early 1837 he was remarried, this time to Mary Lattin, daughter of a well-to-do Lockport man. He was engaged for a while in the shoe business in Haverhill, but shared the fate of many in the depression of 1837, and a few years later returned to Lockport, taking Othniel with him.

Of Othniel's early days, we have but little information. As the eldest boy, he was expected to be his father's mainstay in the farm work, but he preferred to range the countryside, hunting the small game then still abundant in the Lockport region, and becoming an expert shot with a rifle. While he and his stepmother seem to have been on friendly terms, nevertheless the new brood of children, which had increased to six by 1852, the recurring financial troubles, Caleb's inability to cope with them, and his consequent lack of equability, all helped to drive the wedge deeper between father and son. The boy went to school apparently in the winter only, but by 1848 he had acquired sufficient knowledge to allow him to attend Wilson Collegiate Institute at Wilson, New York, where his conduct and progress during the years 1848-1850 were reported by the principal to be "satisfactory". In 1850 he was a pupil in the Lockport Union School, and in that year he tried school teaching, but gave it up because of "headaches"—probably due to near-sightedness. However, he made money enough to enable him to follow a long-cherished wish and go back East; and the next year his doings

at the family homestead in South Danvers are recorded in a diary. This diary, it must be admitted, shows surprising immaturity for a young man of twenty, and yet the year 1851-1852 was the turning point in Marsh's life. Coming of age, he received a settlement, at least in part, of the property held for him since his mother's death (proceeds of the marriage dowry given her by her brother George), and with it, he decided to go to Phillips Academy at Andover, Massachusetts.

He entered the Academy before the turn of the year, and there he found the environment that brought to the fore qualities hitherto dormant. Not at once, however: the first year he did not exert himself, but the second year he settled down to work in earnest. His comparative maturity gave him an advantage over the other boys, and he found within himself a zest for learning. Even more significant, he very shortly developed a talent for leadership. He was at Andover five years, graduating valedictorian in July 1856; according to a statement quoted by Grinnell, "He had made a clean sweep of all the honors of Phillips Academy—there was no desirable honor which he did not get while there."

He had not been in Andover long when George Peabody extended him a helping hand, and in May 1856 Othniel carefully framed a letter to his uncle, expressing his gratitude for past opportunities, and asking permission to enter Yale College. The first actual meeting between the two took place later in that same year, and what George Peabody saw was evidently to his liking, for Marsh entered Yale College in September 1856.

At Yale, Marsh's progress was less brilliant than at Andover, but he was graduated in 1860 (at the age of 28) with a High Orations stand in the Classical Course, eighth in a class of 109. He was elected to Phi Beta Kappa, and he also received a Berkeley Scholarship, awarded for excellence in certain of the classics, and carrying with it the proviso that the recipient must remain at Yale as a graduate student for one to three years. Although his rank as a "Scholar of the House" was thus founded on the classics, Marsh had no intention of following these branches further. Before his graduation he had written his uncle that he wished to fit himself for "a Professorship of Nat-

ural Science in Yale or some other College", and had received the latter's consent. Certainly it was quite in keeping with George Peabody's own career to be willing to foster high ambition when he saw it!

It might be profitable at this point to consider what forces had been shaping Marsh's mind toward science. First among them was doubtless his country upbringing and his great love for the outdoors, expressed in his compositions at the Wilson Collegiate Institute. At the time when the Erie Canal was being excavated, in 1823, and when it was being widened, about 1843, the Lockport region became famous the world over for its Niagaran (Silurian) fossils, and this rich fauna attracted a retired army officer, Col. Ezekiel Jewett (1791-1877), who is said at that time to have been "unsurpassed in America as a field paleontologist." It is known that young Marsh came under the influence of Colonel Jewett some time after 1843—the colonel had a summer school in geology at Lockport in 1843-1847—and learned from him how and where to collect fossils, and especially minerals. By the time he went to Andover, in 1851, he already had a mineral collection of some dimension, and he added to it during five summer trips to Nova Scotia. An interesting item, in view of subsequent events, is the statement in his Andover diary that in April 1855 he called on Benjamin F. Mudge, then curator of the natural history society in Lynn, to see his minerals and to get help in identifying some of his own. During his first two years at Andover, he was in the English Department, which offered some courses in natural science, but with his decision to go to college, made in the spring of 1853, he had perforce to shift to the Classical Course.

After his graduation from Yale College, Marsh turned at once to science, then developing rapidly at Yale under the two Sillimans, George J. Brush, and James D. Dana; and it was under the guidance of these men, and especially of Professor Brush, that he spent two years of study in the young Sheffield Scientific School. As the second year drew to a close, he had to decide what should be the next step toward the professorship that had become his goal. Evidently the idea of study abroad had been in his mind for some time—doubtless placed there by

Brush, who had studied in Germany—for he wrote his uncle on June 9, 1862: "If the plan of completing my studies in Germany, which you once so kindly approved, still meets with your approbation, I should like to go in September next." Mr. Peabody readily assented, and letters of introduction given Marsh by Professor Dana indicate that he was planning to study particularly analytical chemistry and mineralogy.

Marsh started for Germany in November 1862, stopping in London to visit his uncle, to see the International Exhibition, and to look at some fossils in the British Museum that he wished to compare with certain vertebrate remains discovered on his second visit to Nova Scotia some years earlier, and forming the subject of his first paper on fossil vertebrates, "Description of the Remains of a New Enaliosaurian (*Eosaurus acadianus)*", published in July 1862. A copy of this paper had been sent to Sir Charles Lyell for publication in the *Proceedings* of the Geological Society of London, and it was communicated to that Society in December by Lyell himself. In the following year, Marsh was proposed for membership in this leading geological society by Lyell, a signal honor for so young and untried a worker.

Marsh matriculated at Berlin University as a student of mineralogy and chemistry under G. and H. Rose, respectively, and of microgeology under Ehrenberg. In the spring of 1863 he moved on to Heidelberg, to work under Bunsen, Blum, and Kirschoff. During this spring, also, he had a momentous meeting with his uncle while the latter was taking the "cure" at Wiesbaden. This conference was concerned not only with Marsh's hoped-for career, but with Mr. Peabody's "future plans and donations," regarding which the two had had "a long talk" in England the year before. When it was over, Marsh was able to write the elder Silliman that his uncle proposed to give Yale the sum of $100,000 (later increased to $150,000) for a museum of natural science.

After a summer spent in Switzerland, Marsh went back to Berlin in the fall of 1863, and spent the entire academic year in the study of paleontology, at Professor Dana's suggestion; by this time it had been made clear to him that a position in the

Sheffield Scientific School was a probability. The summer of 1864 he devoted to further excursions in Switzerland, and in October he entered the university at Breslau to study with Ferdinand Roemer (who had done much geologic work in Texas and Tennessee), Grube, and Goeppert, returning to Berlin in the spring.

Marsh's formal appointment to the professorship of paleontology in the Sheffield Scientific School—the first such chair in America—was made at Commencement, July 24, 1866. His connection with the Scientific School continued until 1879, when he was transferred to Yale College. According to Grinnell, "he did not wish to make his professorship a teaching one, and preferred to serve Yale without salary in order that his time might be devoted to research and exploration."

MARSH'S PERSONALITY

George Bird Grinnell, naturalist and writer, was one of Professor Marsh's students, a fellow explorer with him on two Yale expeditions in the Rocky Mountain country, and a close friend to the end of Marsh's life. Charles E. Beecher, who succeeded Marsh at Yale, worked in the same room with him for ten years as his assistant, had the full confidence of his superior, and probably understood him better than most other persons, besides knowing more of his later history. The memorials written by Grinnell (1910) and Beecher (1899)[3] form the basis for the following account, supplemented by the writer's own knowledge of Marsh, gained from daily intercourse with him during the year 1892 and occasional meetings during the last six years of his life, and from a study of the thousands of letters received by Marsh, his many notes relating to his own career, an abundance of newspaper clippings, and finally, the nearly three hundred scientific papers that he published.

Marsh's early life as a farmer's son had developed for him a strong frame and a robust body. Never seriously ill at any time in his life, we find him nowhere dwelling on the hardships of life or the dangers and fatigue of field work in his pioneer

[3] Cited, together with other sketches of Professor Marsh, at the end of the bibliography which accompanies this memoir.

days in the Rocky Mountain region. He stood about 5 feet, 10 inches, in his shoes, was stockily built, broad-shouldered, and erect. In early life he probably weighed around 160 pounds, and in later years about 175. In middle life his nose, mouth and chin were average in character, his face round, his complexion fair and of a healthy color. His hair and eyebrows were sandy in tone, with a beard tending toward red. He had widely spaced blue eyes that were somewhat nearsighted—a defect that led to his rejection by the Army in the Civil War days; he wore eyeglasses only while reading and writing, however, and none of his many portraits show him with such. His forehead was high, and a scantiness of front hair caused it to appear even unduly so; with middle life he became slightly bald. As a youth he wore a flowing mustache, but almost all his later portraits show a well dressed full beard.

Although possibly inheriting through his mother many of the traits that were to make him prominent, Marsh grew up without the softening influence that she might have exerted, and his favorite sister, Mary, died while he was in Andover. With his somewhat domineering father he did not get on well. Moreover, as he remained a bachelor, he had no family ties to hold him in check. Self-reliance he possessed to an extraordinary degree, and it naturally led to a self-centering of his life and ambitions. Out of it came, also, Beecher says,

"an absence of the complete exchange of confidence which normally exists between intimate friends. Even where perfect confidence existed, he seldom revealed more about any particular matter than seemed to him necessary or than the circumstances really demanded."

By anyone meeting Marsh for the first time, and especially anyone asking for information in his line of research, the caution of the man must have been instantly felt—possibly even a slightly suspicious attitude until he had made sure that he knew the whys and wherefores of the meeting. Although access to him was easy, the critical visitor soon saw the marked self-confidence that comes with wealth and position. It was but natural that he should be proud of his ancestry, especially of his relationship to George Peabody, who had made his career possible; and he

was very proud indeed of his unique professorship at Yale, as well as of his high standing among the leaders of science. The next things to impress the visitor were minor physical peculiarities, such as the blinking of his searching, nearsighted eyes, and the seeming impatience shown in his nervous, half-articulated "What? What's that?" On longer acquaintance one came to appreciate that his chief characteristic was his feeling that "the work of the hour is of prime importance," and that all those around him should be as interested in it as he was. His ambition to stand at the top is apparent from his Andover days onward, and once he had decided what road to follow, he never wavered in his determination to be one of the highest savants in science and to build up at Yale one of the world's largest foundations in paleontology. He fully accomplished all these wishes.

We get a good insight into Professor Marsh as he appeared to his Yale colleagues in the following excerpt from President Timothy Dwight's *Memories of Yale Life and Men:*

"In his personality, Professor Marsh was, as we may say, a man quite by himself. He was intelligent, with a manly intelligence, and a careful student, patient in his researches. But at the same time, as a collector and discoverer, he had the irrepressible zeal which is characteristic of an enthusiast Every new thing in his own sphere of investigation which revealed itself—everything which had in it the promise of a revelation—gave him happiness and stirred him to fresh activity. He would press forward with all energy, and any needed outlay of effort or means, to secure what it might have to give him. When he had made it his own, and found it of true value, he hastened with joyful ardor to relate his good fortune to his friends, as if he had possessed himself of a hidden treasure. . . .

"In his attitude and in his manner of expressing himself, a certain formality was characteristic of him. Especially was this manifest in cases where he sought an interview with others on matters of business, or on subjects of interest with respect to his own particular work. The slight and somewhat peculiar hesitation in his utterance rendered this formality more conspicuous. I was always struck with this singularity of manner when he called upon me. . . . Whatever the object might be, the manner of the man was the same. It was as if we had been two ministers of state having little acquaintance with each other, who had met for the settlement of some great question of public concern. All was serious with a dignified solemnity, and measured with a diplomatic deliberateness. . . . Such idiosyncrasies made the man the more interesting. They certainly gave him an individuality which distinguished him from others."

Grinnell says that Marsh was a keen judge of men, could instantly select the one he felt would be of most use to him, and was seldom at fault in his estimate of character; that he was efficient and shrewd, with a touch of cunning, and an aggressive leader. Though not an easy writer, "he took great pains to express himself clearly and in correct English."

With the desire to know how Marsh appeared to his European contemporaries, the writer asked this question of Sir Arthur Smith Woodward, of London, whose father, Henry Woodward, was probably the closest of Marsh's friends on that side of the ocean, and from him received the following reply:

"Professor Marsh visited England usually in alternate years, and he had a large circle of friends both in this country and on the European continent. I first met him in 1884 in my second year as an assistant in the Geological Department of the British Museum (Nat. Hist.), and thenceforth I regarded him as one of my best friends. In 1890 I stayed with him in his 'wigwam' (as he termed it) at 360 Prospect Street, New Haven, and I was always closely associated with him during his visits to England. He was absorbed in palaeontological research; and in London he spent most of his time in studying the fragmentary fossil remains of reptiles and mammals in the British Museum for comparison with the much finer specimens which he had at his disposal in America. He visited other cities for the same purpose, and thus had much influence on the progress of vertebrate palaeontology in Europe. . . .

"Like other great men, Marsh had his failings; and close association with him soon revealed both his unrestrained jealousy and his love of popular adulation. His early rivalry with Cope and his later rivalry with Osborn were never-ending subjects of conversation. Flattering newspaper notices pleased him, and I remember he was delighted when the English journal *Punch* published a little picture of him discoursing on the newly discovered skull of *Triceratops* to the British Association at Leeds in 1890. . . .

"Marsh was a remarkably keen observer, and he was quick to see the inferences which might be drawn from the facts before him. He was also one of the foremost systematists of his time, and contributed greatly to the classification of reptiles, birds and mammals. . . . I am convinced that in all essentials Marsh's fundamental contributions to vertebrate palaeontology were his own, and stimulated by his boundless enthusiasm for our science."

In his Academy days, and at Yale College, Marsh learned to be an easy mixer, and his diary shows that this was true not

only among his classmates but also among his instructors; during his vacations he was constantly traveling about, calling on his relatives, on the curators of museums, on local collectors of minerals, and on others whom he had heard of as interested in natural history. This ease in meeting people, and especially notables, stood him in good stead in both America and Europe. The sunny side of his make-up was nearly always uppermost; in Huxley's words, he was "a wonderfully good fellow, full of fun and stories of his western adventures."

Marsh was also very fond of entertaining, and liked to give dinner parties in his finely landscaped home on Prospect Street, which was in reality but another museum in which to display his many trophies, his orchids, his paintings, and his endless examples of Japanese art. Here also he had as a guest, for several days in 1883, the great Sioux leader, Chief Red Cloud, whom he presented to several hundred of the distinguished citizens of the town.

It is a little difficult for a biographer, writing after this lapse of time, to understand fully the trail of hostility that Marsh left in many quarters. Beecher testifies that he was

> "normally restive under restraint, and met all opposition with power and fearlessness. Having practically created the modern science of vertebrate paleontology in America, he resented any encroachment upon the particular fields of research in which he was engaged. This attitude frequently developed feelings of hostility in other investigators, and often alienated him from co-workers in his Department of Science."

Grinnell puts the same idea a little differently, thus:

> "His fossils were priceless in his eyes, and he guarded them with extremest care. A man of less enthusiasm or more liberal mind might have turned over certain subjects to able assistants; Marsh's failure in this respect caused in several cases a rupture of friendly relations. . . . He had one or two unfortunate experiences with visitors; hence was somewhat suspicious. . . . Marsh's peculiarities were many, some of them being so marked as to give his enemies an opportunity to speak ill of him, which sometimes resulted in grave injustice."

Looking back through his career, Marsh appears to the writer to have been a sort of Jack the Giant Killer, for he was forever attacking errors, humbugs, and impostors. He began this in

1861, when he exploded the Nova Scotians' hopes for their gold mines, and in 1862 he corrected no less a savant than Louis Agassiz in regard to a certain feature in *Eosaurus*. Six years later, on his first trip to the West, certain so-called "human" remains found nearly 70 feet beneath the surface in western Nebraska were shown by him to belong to three-toed horses and associated animals; and huge footprints supposed to have been left by giants in ancient Nevada, turned out, under his searching examination in 1883, to be those of a well-known species of extinct ground sloth.

The richest exposure in which he figured was undoubtedly that of the Cardiff Giant in 1869. This was "a gypsum man, ten and a half feet long, nude, virile and unabashed," dug up in the dark of an October night in Onondaga County, New York, and widely shown at fifty cents a head. State Geologist James Hall, inspecting it undisturbed for "a full quarter of an hour," publicly stated it to be "the most remarkable object yet brought to light in this country and although perhaps not dating back to the Stone Age, . . . nevertheless deserving the attention of archeologists." The perpetrators of the hoax were getting rich, when Professor Marsh, suspicious of this gypsum giant from his native state, unearthed its real history with the help of a man from Iowa who, dissatisfied with his share of the profits, declared that he "got up" the giant from a block of Iowa gypsum, and that it was then shipped to Cardiff, New York, hauled by night to its burying place, and "resuscitated with full attention to all necessary details." After his inspection of the giant at Syracuse, during which he made sure that it was composed of gypsum, a substance that is soluble in water and therefore could not have retained, after burial, the polished surface that the statue displayed, Marsh wrote a letter to a newspaper friend exposing all that he had learned, and thus exploded the most uproarious hoax ever "launched upon the credulity of a humbug-loving people" (Clarke, *Life of James Hall,* 1921).

One of the two hardest battles of Marsh's life was his struggle in 1875 with the unscrupulous politicians of the "Indian Ring" at Washington, which started when he sent to President Grant, following a promise made to Chief Red Cloud, a printed

exposé of disreputable frauds that were being practiced upon the Indians at the Red Cloud Agency in Nebraska; and which ended with the cleaning out of the Agency and added the final straw to a load of evidence that had long been accumulating against certain members of the Interior Department. Many of the partisan papers of the time showered the professor with scurrilous language, but so far as Marsh was concerned these attacks had the same effect as water on a duck's back, and in no wise turned him from his determination "to clear out the varmints."

The most protracted and bitter fight of Marsh's life, that against his great rival, Professor Edward Drinker Cope of Philadelphia, broke out in 1873, when he began to point out in print the latter's attempt at antedating papers containing descriptions of new forms of vertebrates. Years of acrimony between the two climaxed on January 12, 1890, when Cope, through a newspaper writer in the New York *Herald,* burst forth with a full-page charge against the doings of Marsh and of Major J. W. Powell, Director of the United States Geological Survey, which was met a week later by a long blast of counter-accusation from Marsh. Of this exchange, Osborn says, in his life of Cope (1931):

> "Cope attacked after a truly Celtic fashion, hitting out blindly right and left with little or no precaution for guarding the rear. . . . Marsh's reply was thoroughly of a cold-blooded Teutonic, or Nordic type, very dignified and, under the cover of wounded silence, reluctantly breaking the silence of years."

During the two closing decades of the last century, nearly all the vertebrate paleontologists in the United States became partisans in this warfare, either openly or otherwise. Reverberations of the quarrel reached the Senate in 1892, when the Geological Survey was under fire because of Major Powell's advanced ideas about irrigation; and it thus became one of the factors that led to the 30 per cent cut in the Survey's annual appropriations (50 per cent in paleontology), and to the consequent discharge of many geologists, aside from Marsh, who had had nothing whatever to do with the controversies.

MARSH AS A COLLECTOR

According to Beecher, it was as a collector that Marsh was seen at his best, and the collections that he amassed during the last thirty years of his life

"form a lasting monument to his perseverance and foresight. . . . He not only had the means and the inclination, but entered every field of acquisition with the dominating ambition to obtain everything there was in it, and leave not a single scrap behind. Every avenue of approach was made use of, and cost was often a secondary consideration. The nine-tenths, when attained, were only an additional stimulus for securing the remaining one-tenth."

This preëminence of Marsh as a collector went beyond the amount of material that he acquired, because the improvement that he made in the technique of collecting was of equal importance. In 1892, when the writer was at work as a preparator of invertebrate fossils in the Yale Museum, he saw masses of rock and bone coming in from the field daily, bandaged and held together by gunny sacking that had been cut in strips, saturated with plaster of paris, wrapped over the fossils before they were lifted out of the ground and again on the under side when they could be turned over. He saw these packages, small and large, opened little by little by the skilful "bone-setters", with no disturbance of the much fractured specimens. As each part was opened, thin shellac or liquid glue was poured into all the crevices, or the parts were lifted one after another and reset in plaster of paris, so that when they dried out, the pieces were held firmly in place. He was fascinated by this resurrection of ancient bones and their preservation in all their structural glory for the edification of paleontologists. Having heard that Marsh was the inventor of the process, the writer asked him one day how this came about. Marsh replied that he got the idea from having seen medical men set broken bones in splints and hold them together with strips of cloth soaked in plaster of paris. This may be true, but rumor has it that the first step in this method was invented by S. W. Williston. In the late summer of 1877, he and M. P. Felch were trying to take up a badly fractured and much weathered *Diplodocus*, and finally, in de-

spair, Williston wrote Marsh on September 21: "Will it do to paste strips of strong paper on fractured bones before removing? . . . These strips are put on with ordinary flour-paste and can be removed I think easily." There is still extant in the Peabody Museum a bone bandaged in this way by Williston and Mudge in 1877. This seems to indicate that the former was the initiator of the bandaging method, and it would be but a step from strips of paper with flour paste to the more secure strips of sacking soaked in liquid plaster of paris. Marsh may have suggested the improvement; in any event, by 1880 this surgical device was in use by all his collectors.

At the time when he began to collect vertebrates, Marsh went on to say, the material that had been described was nearly always of a fragmentary nature, and usually consisted of teeth, broken jaws, and other isolated bones. The truth of this statement is evident at once to anyone who looks through the early publications. As Marsh said, this was the age when the usual method was to drive the pick under the bone, pull it up, rake all the pieces together and dump them into a sack, with the pious hope that the preparators would be able to fit the puzzle together. How much better Marsh and his "bone-hunters" came to do their collecting may be seen from a perusal of the plates in, for example, his monographs on the toothed birds (1880), the Dinocerata (1885), and the dinosaurs (1896), and in his many papers that give lifelike restorations of extinct reptiles and mammals.

John Bell Hatcher, the most extensive collector of Marsh's many field men, speaks of "Marsh's well known aversion to dealing with fragmentary or relatively unimportant material and seeking after the 'choicest plums' as he used frequently to express it." In another place, he adds:

> "Where a generation ago the extinct vertebrate life of America was but poorly represented in our museums by imperfect series of teeth and isolated bones, we are now able to study many of these extinct animals from more or less complete skeletons. For these improved conditions we are mainly indebted to the late Professor Marsh, either directly by reason of the vast collections acquired by him, or indirectly through the improved laboratory and field methods developed by him and his assistants."

The steady stream of vertebrate fossils that poured from the West into the Yale Museum during the years 1870–1873 came as the result of a brilliant idea on the part of Marsh—brilliantly planned and brilliantly executed—in short, the organization of four expeditions into the western territories, the personnel of which was made up mainly of recent graduates or undergraduates of Yale College and the Sheffield Scientific School, who not only did their share of the actual collecting, but paid most of the expenses of the trips. A fifth expedition, in 1874, though rich in yield, was made up of tested collectors and army men. After that date, Marsh did most of his collecting through individual paid collectors and their assistants, directed by correspondence and by occasional trips to the field.

Marsh himself said that at one time or another between 1870 and 1898, and for short or long periods of service, he employed "several hundred" helpers of various types. Included in this category were soldier escorts and their officers and guides, cooks, teamsters, "bone-diggers" and their assistants, and also his laboratory aids. There is no record of those who were thus employed for short times, but more than one hundred persons are represented either by letters or by names in the accession and account books. Of these, at least fifty were employed before 1882, and the remainder during Marsh's connection with the United States Geological Survey; and of the latter, thirty-five were collectors on the federal payroll. The collector *par excellence* was J. B. Hatcher; other efficient ones were B. F. Mudge, Arthur Lakes, M. P. Felch, W. H. Reed, S. W. Williston, Fred Brown, E. Kennedy, D. Baldwin, Gus Craven, Leander S. Davis, Sam Smith, and H. T. Martin. In the years between 1870 and 1892 the vertebrate fossils arrived at New Haven in an endless stream of boxes and packages of all sizes, and at times in carload lots, the accession books recording about 3000 shipments.

The use of his personal fortune and of the funds put at his disposal by the United States Geological Survey made it possible for Marsh to bring together what was probably the greatest collection of Mesozoic and Cenozoic vertebrate skeletons ever amassed by one individual. Cost was never his first considera-

tion—he was after big results. He told the writer that his pet specimen, the giant sauropod dinosaur *Brontosaurus excelsus,* had cost him $20,000, and that he had spent on his collections as a whole not less than $200,000 of his own money. In addition, he spent for the United States Geological Survey about $150,000.

With regard to the improvements in collecting technique brought about by Marsh, we have the testimony of J. L. Wortman, who was in a position to note the changes in methods, since he was long assistant to Cope and later a member of Osborn's large staff in the American Museum of Natural History. In his memorial of Marsh, he says:

"The record of his discoveries is one of almost continual triumph in the bringing to light of new and strange forms of life that had inhabited the western hemisphere in the distant past. . . . The methods of collecting and preparing these fossils for study and exhibition which he has introduced in the course of his long experience form the basis very largely of all similar work in almost every palaeontological laboratory of the world, and it is a matter of common remark that nearly all the noted collectors and preparateurs have received their training under his immediate influence."

Of Marsh's vertebrate collection, presented by him to Yale University in 1898, Wortman said in 1900 that it was

"without doubt the finest and most complete of any in the world, and, when properly installed and exhibited, will make a monument in every way worthy of the greatness of the man who dedicated his life and his fortune to its formation. The influence of his work for advancement in this department of knowledge has probably no equal in any country."

At the same time that Marsh was building up his great collection of fossil vertebrates, he was bringing together, from all parts of the world, skeletons of recent animals to be used for comparative studies. The collection of recent osteological material thus assembled was in Marsh's time one of the most complete in America.

MARSH'S CONTRIBUTIONS TO THE EVOLUTION THEORY

Darwin's epochal work, *The Origin of Species,* did not appear until November 1859, and, since it is well known that Professor

Dana was a creationist until long after that date, it is more than probable that Marsh's attention was not directed favorably toward evolution until his student days in Germany. That he was thinking about it, however, is evident from his paper of 1862 on *Eosaurus,* in which he says that, inasmuch as these bones occur in Paleozoic strata,

"they add another to the arguments that have been brought against the so called 'Development Theory'; and they show with how great caution we should receive the assertions, so frequently and confidently made on negative evidence alone, of the exact date of the creation or destruction of any form of animal or vegetable life."

Marsh made his first visit to England in the fall of this same year, and at this time or shortly thereafter he met Charles Lyell and Thomas Huxley, the latter of whom, he tells us, was "a guide, philosopher, and friend, almost from the time I made choice of science as my life work". We also know that as early as 1865 he had been at the country home of Darwin. In any event, by 1874 he was an out-and-out evolutionist, and in his memorable Nashville address of 1877 he states that evolution is the "key to the mysteries of past life on the earth," and that "to doubt evolution is to doubt science, and science is only another name for truth". In his address of the following year, he adds that Darwin's *Origin of Species* had in two decades "changed the whole course of scientific thought . . . Darwin spoke the magic word—'Natural Selection,' and a new epoch in science began."

The demonstration of the truth of the evolutionary theory can come only through the study of fossils, and Marsh's well preserved and carefully collected material played a large part in the establishment of the hypothesis. It was, indeed, his specimens from the region to the east of the Rocky Mountains, where there exists an unrivaled record of dinosaurs, birds, and mammals, that helped to take the entire question of evolution out of the realm of hypothesis and to demonstrate that it is a living truth.

Marsh's first major contribution to the evidence for the evolution theory was his discovery of birds which, by their possession of teeth and other reptilian characters, proved the

genetic relationship between these two groups of animals that had been foreshadowed by Huxley. It was, however, his magnificent collection of fossil horses, and his accurate and careful tracing, from these, of the progress of the horses through geologic time, that tended to give him a greater reputation than any of his other discoveries.

It has been well said that the living horse is probably the most perfect organic machine for swift running so far developed, and that it displays throughout its organization a most exact and finished adaptation to this purpose. It has, however, taken Nature about fifty million years to perfect this mechanism. The evolution of the superfamily Equoidea "affords the best known illustration of the doctrine of evolution by means of natural selection and the adaptation of a race of animals to its environment" (W. D. Matthew 1913). This evolution begins in the Rocky Mountain country during the Age of Mammals (Cenozoic) in *Eohippus (=Hyracotherium)*, only 11 inches tall, and proceeds by various genetic lines through 15 genera and some 215 different forms into the *Equus* of today.

Even previous to 1865, no fewer than ten kinds of fossil American horses had been described. Nevertheless, as Marsh tells us in his Nashville address:

"I heard a world renowned Professor of Zoology [in Germany] gravely inform his pupils that the horse was a gift of the Old World to the New, and was entirely unknown in America until introduced by the Spaniards. After the lecture I asked him whether no earlier remains of horses had been found on this continent and was told in reply that the reports were too unsatisfactory to be presented as facts in science. This remark led me, on my return, to examine the subject myself, and I have unearthed [many] species of the horse tribe . . . and it is now, I think, generally admitted that America is, after all, the true home of the horse."

In his paper on the fossil horses of America, appearing in 1874, Marsh said:

"The large number of equine mammals now known from the Tertiary deposits of this country, and their regular distribution through the subdivisions of this formation, afford a good opportunity to ascertain the probable lineal descent of the modern horse. The American representative of the latter is the extinct *Eq. fraternus* Leidy, a species almost, if not entirely identical with the old world *Eq. caballus* Linn., to which

our recent horse belongs. Huxley has traced successfully the later genealogy of the horse through European extinct forms [1870], but the line in America was probably a more direct one, and the record is more complete. Taking, then, as the extremes of a series, *Orohippus agilis* Marsh, from the Eocene [Bridger] and *Eq. fraternus* Leidy, from the Quaternary [Pleistocene] intermediate forms may be intercalated with considerable certainty from the thirty or more well marked species that lived in the intervening periods. The natural line of descent would seem to be through the following genera: *Orohippus,* of the Eocene; *Miohippus* and *Anchitherium* of the [Oligocene and] Miocene; *Anchippus, Hipparion, Protohippus* and *Pliohippus,* of the Pliocene; and *Equus*" of the Pleistocene and Recent.

"The most marked changes undergone by the successive equine genera are as follows: 1st, increase in size; 2nd, increase in speed, through concentration of limb bones; 3d, elongation of head and neck, and modifications of skull. The increase in size is remarkable. The Eocene *Orohippus* was about the size of a fox. *Miohippus* and *Anchitherium,* from the Miocene [Oligocene] were about as large as a sheep; *Hipparion* and *Pliohippus,* of the Pliocene, equalled the ass in height; while the size of the Quaternary *Equus* was fully up to that of the modern horse.

"The ancient *Orohippus* had all four digits of the manus well developed. In *Miohippus* . . . the fifth toe has disappeared, or is only represented by a rudiment, and the limb is supported by the second, third, and fourth, the middle one being the largest. *Hipparion* . . . still has three digits, but the third is much stouter, and the outer ones have ceased to be of use, as they do not touch the ground. In *Equus,* the last of the series, the lateral hoofs are gone, and the digits themselves are represented only by the rudimentary splint bones. The middle, or third, digit supports the limb, and its size has increased accordingly. The corresponding changes in the posterior limb of these genera are very similar, but not so manifest. . . . This reduction in the number of toes may, perhaps, have been due to elevation of the region inhabited, which gradually led the animals to live on higher ground, instead of the soft lowlands where a polydactyl foot would be an advantage."

Between 1868 and 1892 Marsh issued thirteen papers relating to fossil horses. His genera, arranged in the order of their description, are: *Orohippus* (1872), *Miohippus* and *Pliohippus* (1874), *Mesohippus* (1875), *Eohippus* (1876 = *Hyracotherium* Owen), *Epihippus* (1878), and *Helohippus* (1892, a synonym of *Orohippus,* as is *Orotherium* 1872).

By 1876, Marsh knew at least thirty kinds of American horses, and most of these he showed Huxley on the occasion of the latter's visit to America. The great scientist was met

by Marsh on his arrival in New York and stayed a week in New Haven as his guest, examining Marsh's general collection, which he wished to see before delivering his course of lectures. Marsh said of this circumstance later:

"One of Huxley's lectures in New York was to be on the genealogy of the horse, a subject which he had already written about, based entirely upon European specimens. My own explorations had led me to conclusions quite different from his, and my specimens seemed to me to prove conclusively that the horse originated in the New World and not in the Old, and that its genealogy must be worked out here. With some hesitation, I laid the whole matter frankly before Huxley, and he spent nearly two days going over my specimens with me, and testing each point I made. He then informed me that all this was new to him, and that my facts demonstrated the evolution of the horse beyond question, and for the first time indicated the direct line of descent of an existing animal. With the generosity of true greatness, he gave up his own opinions in the face of new truth, and took my conclusions as the basis of his famous New York lecture on the horse."

If more striking testimony to the value of Marsh's contributions to the evolution theory is needed, it is at hand in a holograph letter treasured in the Peabody Museum archives, which reads:

"Down, Kent, August 31, 1880.

"My dear Prof. Marsh,—

"I received some time ago your very kind note of July 28th, and yesterday the magnificent volume [Odontornithes]. I have looked with renewed admiration at the plates, and will soon read the text. Your work on these old birds and on the many fossil animals of N. America has afforded the best support to the theory of evolution which has appeared within the last 20 years.[4] . . .

"With cordial thanks, believe me,

"Yours very sincerely,

(Signed) CHARLES DARWIN."

MARSH AND STRATIGRAPHY

When Marsh was receiving his training in geology in the late fifties and early sixties, the stratigraphy of the marine formations was as yet very much generalized, and of the fresh-water deposits there was hardly any known chronology at all.

[4] In other words, since the publication of *The Origin of Species* in 1859.

The time had not arrived when lake and river deposits could be distinguished clearly from those of the seas. Besides, there was little information that was reliable about the genetic sequence of the land plants and the animals found fossil in continental strata. It was but natural, therefore, that Marsh, while a student in Germany, should have been greatly influenced by the dictum of Professor Goeppert of Breslau, who told his classes "to doubt the value of fossil plants as indices of the past history of the world" (Marsh 1898), and that he should unfortunately have retained this opinion throughout his career. It was this prejudice that led him late in life into the grave error of thinking that continental deposits of Jurassic age are present throughout the Atlantic Coastal Plain from Martha's Vineyard and Block Island south into Virginia, despite the fact that the paleobotanists of the United States Geological Survey, on the basis of large floras described by Fontaine and Ward, had shown these deposits of the Potomac group to be of Lower Cretaceous age.

When Marsh began his researches on the Tertiary deposits of the Great Plains east of the Rocky Mountains, it was the general belief that these strata had accumulated in vast lake basins. The first edition of Dana's *Manual of Geology* (1862) held that these formations were of lacustrine and brackish-water origin, laid down in bodies of water that had been of long endurance and that had covered great areas with thick deposits. These erroneous ideas about lake deposits did not begin to break down until the last decade of the nineteenth century. Therefore in Marsh's papers treating of the Cenozoic stratigraphy of the Great Plains we read of "ancient lake basins," and that "the existence of several large fresh water lakes in the Rocky Mountains region, during Tertiary time, is now well established." The lakes, he said, were of Eocene (Wasatch, Bridger, Uinta, Green River, etc.), Miocene = Oligocene, and Pliocene ages. Even as late as 1885, in the monograph of the Dinocerata, he says that these animals are "found in a single Eocene lake basin in Wyoming. . . . This lake basin . . . slowly filled up with sediment, but remained a lake so long that the deposits formed in it, during Eocene time, reached a vertical thickness of more than a mile." That rivers in a semiarid climate lay down their

sediments over widespread flood plains did not come to be general knowledge among stratigraphers until the present century.

In none of Marsh's publications do we find that he ever actually measured or described in detail a stratigraphic sequence. He was content to note the formation in which the fossils occurred and rarely did he mention the subdivision of the formation. He left to the geologist all this most desirable stratigraphic detail, as well as the naming of the formations, since it was his ambition to develop only the biologic sequence which would show the direction evolution had taken. As a rule, his named zones were very broadly conceived.

In his description of new species of fossil vertebrates, Marsh rarely gave exact information as to their geologic level or detailed geographic position, his usual citation being "Upper Eocene of Wyoming," "the Coryphodon beds, or lowest Eocene of Wyoming," "the Miocene of New Jersey," "the Pliocene of Idaho," "the Ceratops beds or the Laramie of Wyoming," "the Atlantosaurus beds of the upper Jurassic in Colorado," and so on. For this lack of detail he was severely criticized by his colleagues, and the statement was often made verbally that his reason was that he did not want others to go to his collecting places and get specimens of his species. In his monograph on birds with teeth, he is more specific than usual as to the geologic horizon; these remarkable Kansas birds, he said, occur "in the Middle Cretaceous [which] corresponds to the strata named by the writer the 'Pteranodon beds' . . . included in subdivision number three in Meek and Hayden's section."

As we have seen, Marsh very rarely attempted stratigraphy from the viewpoint of geology. On the other hand, he was a good biostratigrapher from the standpoint of evolution as seen in the succession of vertebrate life. He was quite correct in holding that vertebrate fossils are of the greatest value in determining the stratigraphic sequence of fresh-water deposits. We may illustrate his principle by the following quotation regarding the evolution of the horse, taken from his paper of 1898:

"Near the base of the Eocene the genus *Eohippus* is found, representing the oldest known member of the horse tribe. Higher up in the Eocene *Orohippus* occurs, and still higher comes *Epihippus*, near the top of the

Eocene. Again through the Miocene more genera of horses, *Mesohippus, Miohippus,* and others, follow in succession, and the line still continues in the Pliocene, when the modern genus *Equus* makes its appearance. Throughout this entire series, definite horizons may be marked by the genera, and even by the species of these equine mammals, as there is a change from one stage to the other, both in the teeth and feet, so that every experienced paleontologist can distinguish even fragments of these remains, and thus identify the zones in which they occur."

Accordingly, it was Marsh's aim to name the successive vertebrate life zones after "the largest and most dominant vertebrate form which characterized them." By this method he had by 1896 named sixteen vertebrate life zones above the Paleozoic, as follows: Triassic, 1; Jurassic, 3 (Hallopus, Baptanodon, and Atlantosaurus); Cretaceous, 2 (Pteranodon and Ceratops); Eocene, 4 (Coryphodon, Heliobatis, Dinoceras, Diplacodon); Oligocene, 3 (Brontotherium, Oreodon, Miohippus); Pliocene, 2 (Pliohippus and Equus); Pleistocene, 1 (Bos). Many of these post-Triassic life-zone terms are still in use for the Rocky Mountain region. Osborn in 1929 proposed a different scheme for the same region, with sixteen life zones.

MARSH AND THE UNITED STATES GEOLOGICAL SURVEY

In view of Marsh's spectacular success in his earlier years as a collector of vertebrate fossils, and the rapidity with which he produced published results, it was only natural, when the reorganized and combined geological and geographical surveys were placed under the direction of Major Powell, that the latter should turn to Marsh for paleontological help. Moreover, through his Yale influence, through his wide personal acquaintance with important men, and through his long connection with the National Academy of Sciences, Marsh could also be very helpful to the Survey in easing the annual appropriations bill through Congress. According to Beecher:

"After repeated solicitations and with promises of material aid in the way of publication and collections, Marsh in 1882 accepted the appointment of Vertebrate Paleontologist to the United States Geological Survey.

This position he held to the time of his death, although the field work for the survey was terminated in 1892. His connection with the Survey gave him increased facilities for publication and for prosecuting explorations in the West."

During the ten years that Marsh was chief of the federal section of Vertebrate Paleontology, the Powell Survey was liberal in allotments for his work, and he was given about $15,000 each year to pay salaries for himself and his numerous assistants—collectors (about 35), preparators (9), scientific aids (8), and artists—and for field and laboratory expenses, including large freight bills.

The monograph of the Dinocerata was published by the Survey in 1886, and those of the vertebrates of the Denver Basin and on the dinosaurs of North America in 1896, but none of the five other monographs projected was completed. The material for them had been collected, most of the plates (about 215) and many text figures had been made, and preliminary descriptions of the genera and species had been published, but the final detailed accounts and the philosophical and phylogenetic problems were left largely untouched. As Beecher said, Marsh "planned his life work on the basis that immortality is here and not in the hereafter. It seemed difficult for him to realize the limitations of human existence and worldly accomplishment."

The material results of the ten years' service given to the federal Survey by Marsh and his staff amounted to seven carloads of vertebrate fossils. Shipments totaling two carloads (255 large boxes) were sent to the United States National Museum in 1886, 1891, 1896, and 1898. The final sending, made in 1899 after Marsh's death, filled five freight cars (529 boxes). Walcott, referring to the cessation of Marsh's relations with the Survey, said in 1900 that the value of these collections

"will be upwards of $150,000. . . . The transfer of these great collections to Washington without the loss of any material, either through imperfect recording or through misunderstanding as to the ownership of specimens, reflects the greatest credit on the business-like methods and the integrity of Professor Marsh."

PRESIDENT MARSH OF THE NATIONAL ACADEMY OF SCIENCES

Professor Marsh was greatly elated over his election to membership in the National Academy of Sciences at the April meeting of 1874 in Philadelphia, which was announced to him in a congratulatory telegram by John Strong Newberry. He was then forty-three years old, in the eighth year of his professorship at Yale; and the number of his papers describing striking new discoveries in the fossil fields of the West was just short of fifty. The geologists in the Academy at that time were a strong and influential group, including as they did Cope, Dana, Guyot, James Hall, Hayden, E. W. Hilgard, Hunt, Leidy, Lesley, Lesquereux, Meek, Newberry, Pumpelly, W. B. Rogers, and Worthen. Prior to his election, he had made one personal appearance before the Academy, by invitation, when he read a paper at the Northampton meeting in September 1868 on "human" bones found in a well in Nebraska, which had turned out to be those of an upper Miocene (Loup Fork) horse and other contemporary mammals. In the spring following his election he read his second paper, on "Size of the Brain of Extinct Mammals," and in his twenty-five years of membership, he read in all eleven papers before the Academy.

Marsh became an officer in the Academy in 1878, when he was elected vice-president. Alexander Bache, the founder-president, elected in 1863, had not lived to complete his six-year term, and Joseph Henry, who had succeeded Dana as vice-president in 1866, served as acting president until 1868, when he in turn was elected president, and was re-elected in 1874. Following Henry as vice-president had been William Chauvenet (1868-1870) and Wolcott Gibbs (1872-April 1878). At the spring meeting of 1878, Gibbs gave way to Marsh as vice-president, and on May 13, 1878, President Henry died and Marsh thus became the Academy's acting president four years after his election to membership. He held this office until the spring meeting of 1879 when, with the election of W. B. Rogers to the presidency, he resumed his status as vice-president. Curiously, however, the third president, like the other two, was not destined

to fill out his elected term. Rogers died in 1882, and once more Marsh was acting president. In April 1883, Wolcott Gibbs was elected to succeed Rogers, but as he could not serve, the presidency was given to Marsh. As a result of this odd chain of circumstances, Marsh was no stranger to the Academy's administrative duties when he became its presiding officer. That he took his duties seriously is evident when he tells us that in his seventeen years of administrative service he did not miss a single stated meeting of the forty called, nor was he absent from any of the meetings of committees of which he was a member. No other member of the Academy, either before or since his time, had as long or as strenuous a service as its presiding officer.

In 1881, the Academy had been in existence for eighteen years, and during this time no fewer than 649 papers had been read at the scientific sessions. Of these papers, only five had been published by the Academy, and President Rogers felt that the organization had not received the recognition by the scientific world that would have come if the papers of each year had been published promptly by the Academy. He proposed that they should be brought together and transmitted with the annual report to Congress, but nothing came of this move. During Marsh's presidency, the plan of issuing a volume of papers each year or two became fairly well established, six volumes (in eight parts) appearing up to 1895; and during his term there was no lapse in the publication of the annual reports.

As a presiding officer, Marsh exercised the same amount of care that he bestowed on his private affairs, and he was an active and efficient leader. Testimony as to this is included in a statement from Professor Russell H. Chittenden, a member of the Academy since 1890, who, at the writer's request, wrote down the following recollections:

"As president of the Academy during a period of twelve years, 1883–1895, Professor Marsh through his strong personality exerted an influence on the meetings of the Academy which resulted in a dignified formality in keeping with its high standing. Somewhat stern in appearance, rather punctilious in intercourse with his associates, and with a stiffness of bearing frequently misunderstood, Marsh nevertheless possessed an innate courtesy and kindness of heart which softened his

apparent hardness and made him a friend and colleague to be respected and admired.

"His interest in the Academy was deep and sincere. By many means, both direct and indirect, he sought to promote its standing with the administration at Washington, even suggesting to the President specific ways in which the Academy could be of service to the Government. Whenever possible at the annual meeting in Washington he arranged for a formal call of the Academy on the President at the White House, this at a time when there was laxity in this custom. In these and other ways he used his office to enhance the standing of the Academy as an efficient servant of the Government of the United States."

Between 1881 and 1883, Marsh had much to do with placing the administration of the Academy's trust funds on a secure basis. As he said in his address to the Academy on April 19, 1889:

"The Academy may justly congratulate itself on the possession of its trust funds for the promotion of original research in science. Three of these, for discoveries in astronomy alone, are recent gifts to the Academy, and already the Draper, Smith, and Watson gold medals, the first fruits of these donations, have been awarded, and promise to do much to encourage future study. The acquisition of these gifts made it necessary for the Academy to secure from Congress the authority to receive and hold trust funds in aid of scientific investigations, and this was accomplished in June 1884."

The first award of a medal took place in 1886, when Samuel P. Langley was given the Henry Draper medal. In the following year the Watson medal was awarded to B. A. Gould. The J. Lawrence Smith medal was presented for the first time on the evening of April 18, 1888, in the lecture room of the U. S. National Museum, the recipient being Professor H. A. Newton. This meeting was made still more memorable by the presentation of the second Draper medal to Professor Edward C. Pickering. In Marsh's time ten medals in all were given.

In 1883, the Academy began the practice of sending delegates to other learned societies and to universities, both in this country and in Europe, Marsh being delegated in 1892 to represent the Academy at the tercentenary of the University of Dublin.

As is well known, the one thing that sets the National Academy apart from other scientific organizations in America is its rela-

tion to the Government. The Academy's historian, F. W. True, states this in the Jubilee volume of 1913 in these words:

> "Other scientific organizations were founded whose membership was drawn from all parts of the country, whose scope covered all branches of scientific research, and whose transactions reflected credit on their membership and on American science, but none could claim recognition as the scientific adviser to the Government."

The Academy was founded for this purpose, and its constitution was framed with this in view. Nevertheless, in the fifteen years previous to Marsh's administration, although committees had been called on twenty-seven times by the various Government bureaus for reports on technical matters, Congress itself had turned to the Academy but twice, once in 1869–1870 when the Academy was directed to draw up plans for the scientific operations of the *Polaris* expedition to the Arctic, and once in connection with the Transit of Venus Commission in 1874. During Marsh's official terms (vice president or acting president 1876-1883, and president 1883-1895), the Academy was asked for advice by Congress three times, indirectly by Congress once, and by the departments thirteen times. The work of all these committees is described in the Jubilee volume. From Marsh's time up to the Jubilee year (1896-1913), there were but five calls, a falling off of direct service that is ascribed by True to "the increase of large scientific organizations in the country, the growth of public opinion relative to scientific matters of more or less practical importance, and the development of the scientific bureaus of the Government."

Regarding this relationship of the Academy to the Government, Marsh said in 1889:

> "The question has arisen, shall the Academy, in addition to the duty of giving advice when asked, volunteer its advice to the Government? Members of the Academy have urged this course at various times in the past, and during the present session the question came up again for decision. My own opinion on this subject, after careful consideration, is against such action. The Academy stands in a confidential relation to the Government, as its scientific adviser, and in my judgment it would lose both influence and dignity by offering its advice unasked.
>
> "In appointing the committees on the part of the Academy, I informed them that the proper province of the National Academy is not merely to

make a technical examination in any case, but especially to bring out the scientific principles involved in the investigation, as a basis for future use."

Probably the most important, and certainly the most controversial, affair that came before the Academy in Marsh's period of administration was the reorganization of the various geological surveys, which was referred to the Academy by an act of Congress approved June 20, 1878, under the title, "On a Plan for Surveying and Mapping the Territories of the United States". This was concerned with the relative merits of military or civil control of public enterprises centering around the surveys of the public domain, and the discussion of it, begun as early as 1869, had become animated, acrid, and widespread. The Academy's solution of the problem was to have far-reaching results, not only for American geology in general, but for Marsh himself, whom that curious chain of circumstances hitherto noted had brought to the acting presidency of the Academy at this particular time.

The state of affairs that led up to this request by Congress has been described by George P. Merrill, in *The First One Hundred Years of American Geology* (1924), thus:

"The period of the Civil War had brought to light a considerable number of men for whom the piping times of peace, even when varied by Indian outbreaks in the West, afforded insufficient opportunities. They were men in whom the times had developed a power of organization and command. They were, moreover, men of great physical and moral courage. It was but natural, therefore, particularly when the necessity for military routes in the West and public land questions were taken into consideration, that such should turn their attention toward western exploration. . . . Willing workers were abundant and Congress not difficult to persuade into granting the necessary funds. Hence expedition after expedition was organized and sent out, some purely military, some military and geographic, with geology only incidental, and others for the avowed purpose of geological and natural history research."

As a result of this condition, there were functioning in 1874 six separate surveys of the western territories—two geological surveys under the Engineer Corps of the Army and two under the Department of the Interior, a land-parcelling survey under the latter department, and the United States Coast and Geodetic

Survey under the Treasury Department—all with little or no attempt at mutual collaboration, more or less overlapping, and consequent wasteful expenditure of time and of public funds.

The discussion finally became so acute that Congress decided reform must be brought about, and set it in motion with the following act:

"And the National Academy of Sciences is hereby required at their next meeting to take into consideration the method and expenses of conducting all surveys of a scientific character under the War or Interior Department, and the surveys of the Land Office, and to report to Congress as soon thereafter as may be practicable, a plan for surveying and mapping the Territories of the United States on such general system as will, in their judgment, secure the best results at the least possible cost; and also to recommend to Congress a suitable plan for the publication and distribution of reports, maps, and documents, and other results of the said surveys."

When this act was approved on June 20, 1878, Marsh was in Europe. Upon his return in August, after consulting members of the Academy Council and others, he at once set about fulfilling the wish of Congress. In his own words, as given in his annual report for that year:

"I was required to appoint a special committee to consider the subject. The report of the committee, when completed, could in accordance with the constitution of the Academy . . . be transmitted directly to the government, and afterward to the Academy at its next stated session. Inasmuch, however, as the subject to be considered was of great importance, I thought it better to have the report submitted first to the Academy before transmission to Congress.

"In the appointment of this special committee it was obvious that I could not properly select as members any of those who had taken part in the controversy between the then existing government surveys; which contention, it was said, had resulted in the passage of the law for the proposed reorganization. Again, the subjects to be considered by the committee pertained to mensuration, geology, and natural history, and I therefore selected those who were familiar with these branches of science."

The committee appointed by Marsh was made up of the following Academicians: James D. Dana, William B. Rogers, J. S. Newberry, W. P. Trowbridge, Simon Newcomb, and Alexander Agassiz. Their appointment, True says,

"led to a protest by General Humphreys, Chief of Engineers, who asserted that 'a properly constituted committee should have had among its members

those officers in the Government service whose duties consisted in part or in whole in making geodetic, topographic, or other scientific surveys in the different departments of the government.'"

Marsh then went on to say:

"As the surveys under the War Department and the Interior Department were the special subjects for investigation, I addressed letters to the Secretary of War and the Secretary of the Interior informing them that a committee of the Academy had been appointed to consider the matter, and requested any information as to their plans or wishes in regard to the scientific surveys under their departments they might think proper to lay before the Academy. In reply, the Secretary of War sent a communication from the acting Chief of Engineers of the Army, and the Secretary of the Interior sent reports from the Commissioner of the General Land Office, from Prof. F. V. Hayden, and from Maj. J. W. Powell, all of which were carefully considered by the committee."

The committee deliberated some three months, then handed in a report of about 2000 words which was brought before a special meeting at the autumn session of the Academy in New York City, November 6, 1878, "and after a full discussion of three hours was adopted with only a single dissenting vote". Acting President Marsh then transmitted the report to the President of the Senate and to the Speaker of the House of Representatives.

The report recommended the recombination of the various surveys into three: 1. Coast and Geodetic Survey, "whose function will embrace all questions of position and mensuration"; 2. U. S. Geological Survey, to determine "all questions relating to the geological structure and natural resources of the public domain"; 3. Land Office, to control "the disposition and sale of the public lands": all three organizations to be within the Department of the Interior.

When the report was printed, the chief opposition to it came, as was to be expected, from the War Department, and especially from General Humphreys, Chief of Engineers, but despite this the House Committee on Appropriations incorporated the whole plan proposed by the Academy in a bill (House Res. 6140) which was duly reported to Congress. The final action by Congress, however, accepted only that portion of the plan relating

to the establishment of a single geological survey under the Department of the Interior, and appointed a commission to consider the codification of laws relating to the survey and disposition of the public domain, leaving the matter of the mensuration surveys for the present in abeyance.

Another important recommendation of this report, adopted by Congress, was that

> "All collections of rocks, minerals, soils, fossils, and objects of natural history, archaeology, and ethnology, made by the Coast and Interior Survey, the Geological Survey, or by any other parties for the Government of the United States, when no longer needed for investigations in progress shall be deposited in the National Museum."

This provision was added by Marsh.

Among the reports made by Academy committees to various departmental bureaus during Marsh's terms of service, one of the most discussed was that on glucose, presented in 1882 by Remsen, Chandler, and Barker, which covered 77 printed pages. With this report Marsh had nothing to do directly, but he was in frequent correspondence with the committee during its deliberations.

Marsh's presidency of the Academy came to an end in April 1895, when he refused re-election for a third term. In his farewell address, on April 19, he said in part:

> "In conclusion, allow me to congratulate the Academy on the substantial progress it has already made, the sure foundation on which it now stands in its relations to the Government, and its high position in the ranks of the scientific societies of the world. . . . I am especially grateful for the unanimous vote of thanks by which you have set the seal of your approval on my services as vice-president and president of the Academy during the last seventeen years."

It was Marsh's wish that "the influence of the Academy should be scrupulously reserved for the promotion of noble ends." At his death in 1899, it was found that he had bequeathed to the Academy in his will the sum of ten thousand dollars "for promoting original research in the natural sciences," a sum that was later increased to twenty thousand dollars by the trustee of his estate.

MARSH'S WORK IN VERTEBRATE PALEONTOLOGY

The Astonishing Dinosaurs

Of all land animals that ever lived, none was more remarkable than the dinosaurs, and they were certainly the most wonderful creatures discovered in Marsh's time. They were the masters of the warmer parts of the continents during the Mesozoic era, and for about one hundred and thirty million years they were the rulers of all life. The Age of Reptiles saw their rise, culmination, decline, and extinction.

In general form, dinosaurs were more like mammals than any other class of animals, not sprawling as do most reptiles, but standing well up on their legs; nevertheless, they were the most specialized of all reptiles. Most significant was the smallness of their brains. It mattered not whether the head in these giants was a foot or eight feet in length, the sensory center weighed but a few ounces, or at most two pounds, in bodies that in some forms weighed as much as forty tons. Truly, the long Age of Reptiles was characterized by low mentality and brute strength.

Isolated bones and partial skeletons of carnivorous dinosaurs are now known to have been found as early as 1820 in the Upper Triassic strata near East Windsor, Connecticut; and in 1824 one of the duck-billed dinosaurs, *Iguanodon*, was described from the late Jurassic deposits of England, although the relationships were then unknown. Curiously, the numerous so-called "bird tracks" of the Connecticut Valley did not attract much attention until 1835, and it was many years later before these highly varied foot impressions were proved to be the tracks of dinosaurs. By 1841, enough different kinds of dinosaur remains were known to show the leading British anatomist, Sir Richard Owen, that these animals were different from all other known reptiles, and he proposed the name Dinosauria ("terrible reptiles") for them, regarding them as an order.

In this country, bones and teeth of small sauropod dinosaurs were found in 1858 in the Lower Cretaceous near Bladensburg, Maryland, and a single tooth of a different type was found

about this same time by Hayden in the Judith River region of Montana. Still later, remains of *Iguanodon*-like dinosaurs were recovered in the marl pits of New Jersey farmers and elsewhere along the Atlantic border.

The possibility of the Great Plains as a major source for dinosaur remains may have suggested itself to Marsh on his first transcontinental trip, in 1868, when he was shown a sauropod bone found near Lake Como, Wyoming; but at that time he was apparently too much engrossed in other things to follow up the discovery. Late in 1870, in the marine strata of the Upper Cretaceous of western Kansas, he found the greater part of a small dinosaur skeleton that he named *Hadrosaurus* (now *Claosaurus*) *agilis*. It was of the same general type as those from the New Jersey marl pits, but only one or two other specimens of this species have been found since. A few years later, Cope brought back from the Judith River region of Montana a long series of dinosaur teeth (21 species), indicating that dinosaurs had been present there in abundance, and that some of them had reached considerable size. From that time on, Marsh was on the alert for dinosaurs, and he kept himself posted as to new finds through the newspapers of the time and through correspondence with military officers and scouts stationed at the frontier posts east of the Rocky Mountains. Partly as the result of this vigilance, there were two periods in his career when the dinosaurs of the Great Plains were on the march to New Haven in carload lots. The first of these "migrations" was of late Jurassic forms, and it began in 1877 and lasted strongly until 1886; the other, of late Cretaceous forms, began in 1888 and continued until 1892. These discoveries were so remarkable that the story may be told in some detail.

Late in the spring of 1877 Marsh received a most welcome letter from Arthur Lakes, an Englishman teaching school at Denver, stating that he and Captain H. C. Beckwith, U. S. N., were digging out, at Morrison, Colorado, the bones of a gigantic animal which they wished to sell. As Cope had also been informed of this find, Marsh rushed one of his experienced collectors, Professor B. F. Mudge, to Lake's quarry, and bought all the bones (including even the few remains that had been sent

to Cope for inspection), announcing in July 1877 the discovery of "a new and gigantic dinosaur", which he named *Titanosaurus* (later *Atlantosaurus) montanus*. This striking new discovery was widely heralded in the newspapers of Denver and elsewhere, and other collectors were quickly at work. Mudge was sent to Canyon City to inspect another promising locality, and after his report, Marsh transferred Samuel W. Williston, one of Mudge's most alert students, from western Kansas to Colorado. Williston went to work with a will on September 21. It was only a short time, however, before he wrote Marsh that Cope's man, O. W. Lucas, was getting "by far the best lot of fossils," and that another man was out prospecting all the time for Cope. Young and impatient, Williston also set out to find "better bones," and by October 27 had gone as far north as Morrison. This battle of prospectors continued until Williston was ordered by telegram to go at once to Como, Wyoming, to look at still a third "bone-yard". From this place he wrote Marsh on November 14 that he had struck pay dirt with a vengeance, that well preserved bones could be had there "by the ton. . . . Canyon City and Morrison are simply nowhere in comparison with this locality. . . . I shall commence work . . . about 250 yards from the northwest shore of Como Lake."

The finding of these many dinosaur localities in Wyoming and Colorado finally led to the employment by Marsh of dozens of "bone-diggers", and during the years 1877 to 1886, Marsh and the United States Geological Survey spent there upward of $10,000 a year. In the course of these ten years about 134 small packages, mainly with mammal teeth, and at least 480 large boxes of dinosaur bones came from Como alone; in addition, Marsh received about 270 boxes of dinosaur bones from Canyon City, and 230 from Morrison. All in all, the late Jurassic formations—Marsh's Atlantosaurus beds—yielded him not less than 1115 boxes, large and small, of dinosaur remains. Out of this material he described 21 new genera and 41 new species, truly the richest harvest of dinosaurs ever garnered by a single paleontologist. Among them were the largest members of the order, *Brontosaurus* and *Diplodocus,* the carnivorous horned

Ceratosaurus, and also the most bizarre of dinosaurs, *Stegosaurus*.

The swamp-living sauropods, ponderous quadrupedal dinosaurs of world-wide distribution in late Jurassic time, included the largest land animals that ever lived, in fact, "greater than was supposed possible in an animal that lived and moved upon the land". One of the mightiest of these, the "thunder saurian," *Brontosaurus excelsus*, was described by Marsh from the most perfect sauropod skeleton ever dug up, found by Reed near Como. When the bones were discovered, Marsh states, the huge skeleton "lay nearly in the position in which the bones would naturally fall after death." In August 1883 he presented a lifelike restoration of this skeleton, the first to be published of any dinosaur, and this illustration, together with many other drawings based on his work, has gone into many textbooks. The *Brontosaurus* skeleton, as now mounted in the Peabody Museum of Yale University, is 67 feet long and 16 feet high at the hips, and the animal is estimated to have weighed nearly 30 tons in the flesh. The head is less than 2 feet long, "smaller in proportion to the body than in any reptile hitherto known," and the astonishingly small brain weighs less than one pound, while the enlarged neural ganglion in the sacrum is about three times as large. An Indian elephant ("Rya"), by contrast, had nearly 11 pounds of brain to 4 tons of body weight. A slenderer and lighter sauropod associated with *Brontosaurus* was *Diplodocus*, which had a length of nearly 80 feet to the end of its long whiplike tail.

The overspecialized stegosaurs of the late Jurassic of Wyoming and Colorado, first described by Marsh in 1877, were provided with a mighty armor composed of huge plates and long spikes, which Marsh placed in a single row down the back and tail. This curious armature, Marsh says, "could not have been anticipated and would hardly have been credited had not the plates themselves been found in position." Now, however, nearly all vertebrate paleontologists agree that these plates were arranged in two alternating rows along either side of the dorsal processes of the vertebrae, while the tail spikes were opposite, in pairs. These plates and spikes appear to have been for

defense against the terrible carnivores of the time. *Stegosaurus* was more than 18 feet in length and about 10 feet tall; the beaked head was only about 17 inches in length, with a brain 5 inches long and weighing 2½ ounces or less, to control 10 tons of animated flesh.

Marsh's second period of dinosaurian discovery was concerned with the horned ceratopsians, whose evolution was recorded in about 3500 feet of late Cretaceous strata.

The Ceratopsia were again huge and ponderous dinosaurs, ranging up to 23 feet or more in length and up to 8 feet in height, with skulls from 4 to 8 feet long. The brain in these huge heads was not over 6 inches long and "smaller in proportion to the entire skull than in any known vertebrate." The jaws were provided with a sharp cutting beak, and in *Triceratops* there was a strong horn on the nose, a pair of very large pointed horns on the top of the head, and a row of sharp projections around the margin of the posterior crest. This huge, expanded crest, which overshadowed the back of the skull and neck, was evidently of secondary growth, a practical necessity for the protection of the neck and even more so for the attachment of the powerful ligaments and muscles that supported the great head.

The region from which nearly all the best ceratopsians come is an area about 15 by 35 miles in the east-central part of Converse [now Niobrara] County, Wyoming. In this area Hatcher, with the help of O. A Peterson, W. H. Utterback, A. L. Sullins, W. H. Burwell, and Charles E. Beecher, worked for most of four years, shipping to Marsh more than 300 large boxes, containing 31 "big skulls" and several fairly complete skeletons of horned dinosaurs, besides much other material among which were more than 5000 small mammal teeth and jaws. The largest ceratopsian skull shipped was No. 24, which, with its box, weighed 6850 pounds. The amassing of this great amount of material was, as Osborn says, one of the greatest achievements of Hatcher's remarkable life.

In the revision of the Ceratopsia made by Hatcher and Lull in 1907, we learn that of the 13 genera proposed by Marsh, Cope, Lambe, and Lull, but 6 were left in good standing within the

group, Marsh losing 2, Cope 4, and Lambe 1. Lull's memoir of 1933 recognizes 6 genera, 3 of which belong to Marsh.

In addition to the ceratopsians, Converse County yielded other dinosaurs, duck-billed and ostrich-like forms, both bipedal running types. The duck-bills (*Claosaurus* = "trachodonts") were characteristic animals of late Cretaceous time the world over, and lived along the shores of rivers and lakes. The front of the mouth had no teeth but the jaws had a horny cropping beak as in birds. Marsh's best material in this group came from Wyoming, where two entire skeletons were found, each with a length of about 30 feet and a height of 15 feet.

Marsh's work on the Dinosauria is recorded in 55 papers and books. These were issued between 1872 and 1899, and most of the results of the first 50 papers are summarized in his quarto monographs, *The Dinosaurs of North America* (1896) and *Vertebrate Fossils of the Denver Basin* (1896). In these publications Marsh named and described 80 new species (2 lost to Leidy),[5] and these he classified in 34 new genera, of which 27 are still in use (1 each lost to Leidy, Cope, and Johnston, and 4 to himself), in 7 new suborders, and 3 new orders—surely an astonishing record for one man to make in less than thirty years. Even more valuable than the descriptions is the perfection of much of the material left to posterity, since among the several hundred individuals studied by Marsh many are represented by almost complete skeletons. All this material is now in the United States National Museum, or in the Peabody Museum of Yale University.

In 1895, Marsh was not disposed to accept the view that the Dinosauria belong to two or more distinct groups, each of independent origin, because of "the very limited information we now have in regard to so many dinosaurs known only from fragmentary remains." The tendency among more recent authors, however, is to abandon the term Dinosauria, since it surely includes reptiles of two phylogenetic lines.

[5] The taxonomy followed throughout this paper is that used by O. P. Hay in his catalogues of fossil vertebrates, with one or two exceptions.

Flying Reptiles

Late in November 1870, after an especially successful day's fossil hunting in the chalk bluffs of the Smoky Hill River in western Kansas, Marsh was returning to camp with his soldier escort, when he espied from the saddle a fossil bone. The lateness of the day made it impossible for him to explore the rock further, but he saw that the bone "was hollow, about six inches long and one inch in diameter, with one end perfect and containing a peculiar joint that I had never seen before." Placing the bone in his "softest pocket," he re-examined it in camp and thought it might prove to be the tibia of a gigantic bird. Back in New Haven, with figures of other material for comparison, he saw that the bone was from the wing finger of a pterodactyl, the first remnant of these flying reptiles to be found in America, but of a much larger species than any known European form. He at once asked himself how great must have been the wing expanse of this animal when alive, and concluded that it "would be about twenty feet . . . truly a gigantic dragon even in this country of big things."

In June 1871, Marsh hurried back to the Smoky Hill River country and to the spot where he had found the bone the previous autumn. Dismounting, he found the impression of the very bone he had collected, "and following it up with great care, I obtained the upper end of the same bone." To his great joy, further digging turned up another joint which fitted onto the first one, and directly he "uncovered still another bone, and at last the whole series that supported the gigantic wing of the ancient dragon." These he measured roughly and determined that "this first found American dragon was fully as large as my fancy had painted him."

As all pterodactyls known up to 1876 were provided with teeth, Professor Marsh must have been greatly surprised to learn that his Kansas forms had none at all. In view of the fact that they showed characters so widely different from all other forms in the Old World, he proposed a new order for the American types this same year, calling it Pteranodontia, and the family, Pteranodontidae, from the typical genus *Pteranodon*. The nearly

perfect skull and jaws of *P. longiceps,* he says, "are more like those of birds than of any known reptiles," and the head of *P. ingens* was no less than 4 feet long. The tail was slender and short, and the posterior limbs, though small, were well developed.

In the decade beginning with 1871 Marsh described and named, of the order Pterosauria, 3 new genera (1 a synonym), and 8 new species (1 preoccupied) from the Cretaceous of Kansas, and 1 new genus and 1 new species from the Jurassic of Wyoming. After 1876, so many extraordinary fossils crowded in upon him that he never was able to write a detailed account of his pterodactyls. This was well done later on, however, by Williston, in eleven papers published between 1891 and 1911, and by George F. Eaton in his *Osteology of Pteranodon,* 1910. From the latter memoir we learn that the spread of wing in the usual run of pteranodons was between 11 and 16 feet, but that the largest, *P. ingens*, shows a breadth of 22 feet, 3 inches, and single large bones indicate a probable maximum of 26 feet, 9 inches. The bones are a marvel of lightness, and Williston estimates that the entire animal, when alive, did not weigh more than 30 pounds. This combination of great wing expanse and extreme lightness of body was of much interest to Secretary Langley of the Smithsonian Institution in his pioneer aviation studies, and it was at his request that F. A. Lucas, of the Museum staff, made a reconstruction of *Pteranodon.*

In addition to his American finds, Marsh also secured one of the most perfect pterodactyls ever unearthed in Europe. This was found in 1873 by Martin Krauss in the late Jurassic lithographic limestone of Eichstätt, Bavaria, and there was great rivalry as to who was to have the specimen, which was of the long-tailed type with an abundance of teeth. Marsh cabled his friend, Prof. H. B. Geinitz, to secure this remarkable fossil for him, paying about $1000 for it and other Solenhofen fossils. It is now one of the gems of the Peabody Museum. It belongs to the genus *Rhamphorhynchus*, and Marsh gave it the specific name *phyllurus.* The bones of the skeleton, he says,

"are nearly all in position, and those of both wings show very perfect impressions of *volant membranes* still attached to them. Moreover, the

extremity of the long tail supported a separate vertical membrane, which was evidently used as a rudder in flight."

Marine Lizards (Mosasauria)

Another group of reptiles to which Marsh made a considerable contribution was the mosasaurs, descendants of carnivorous land reptiles that at some time in the early Cretaceous, and at some place as yet unknown, had invaded the realm of the seas. So common that they were supreme rulers of the Upper Cretaceous seas, they grew to lengths of 30 and exceptionally of 40 feet, with skulls as long as 5 feet, and with backbones made up of more than 100 vertebrae. "On one occasion," Marsh says, "as I rode through a valley washed out of this old ocean bed, I saw no less than seven different skeletons of these monsters in sight at once." By 1880 he had at Yale bones representing not less than 1400 individual mosasaurs, collected, for the most part, by Williston, who became the leading authority on the group. Marsh himself, in eight brief papers, described 7 new genera (2 lost to himself and 2 to Cope) and 18 new species.

Birds with Teeth

The remains of birds are among the rarest of fossils. Their bones, other than those of leg and wing, being very frail, are soon destroyed by the atmosphere, by being crushed, or in other ways; and the buoyancy of their bodies, when afloat, causes the winds and waves to carry their cadavers shoreward where they are sure to be devoured by carnivorous or scavenging animals.

At the outset of his fruitful career in paleontology, Professor Marsh was well aware of these facts, and he was constantly on the hunt for bird remains. His first single bones he secured from New Jersey, in deposits considered at the time to be of Cretaceous age but recently placed in the Eocene; describing them in 1870 as 3 new genera and 5 new species of aquatic birds, he regarded them all as representatives of families now living.

In December of that same year, in the course of the first of his western explorations with a party of students, Marsh found a bird tibia in the Niobrara (Cretaceous) chalk of western Kansas, and it whetted his appetite for better material. The

next year's party, hunting in the Smoky Hill region of the same state, was primed to look for fossil birds; and on November 29 Marsh wrote Professor Dana from San Francisco that among their season's trophies was the headless skeleton of a great bird, found by himself, and parts of four other individuals, found by his students; "on my return," he adds, "I shall describe this unique fossil under the name *Hesperornis regalis*," a promise which he made good in the *American Journal of Science* for January 1872.

The following summer Mudge was collecting in the Smoky Hill country, and Marsh, aware of this, wrote him on September 2 to inquire about his results. Mudge, well disposed toward Marsh from former acquaintance, "practically presented" him with a box of his fossils, regarding which Marsh wrote him on September 25 that "the hollow bones are part of a bird, and the two jaws belong to a small saurian. The latter is peculiar, and I wish I had some of the vertebrae for comparison with other Kansas species." Under this belief, the specimens were described by Marsh as pertaining to different animals, the avian bones being named *Ichthyornis dispar* in October 1872, and the "saurian" bones receiving the appellation *Colonosaurus mudgei* the following month. Not until further preparation of the specimens had revealed a skull and additional portions of both jaws did it become apparent that all the bones belonged to one animal, and that animal a *bird with teeth*. This remarkable discovery was announced in February 1873, in a preliminary paper in which the author modestly remarks that "The fortunate discovery of these interesting fossils . . . does much to break down the old distinction between Birds and Reptiles, which the *Archaeopteryx* has so materially diminished." Williston later spoke of these fossils as "by far the most important specimens of these early years [of Marsh's career], if not the most important of those succeeding," and Osborn in 1931 remarked that they constituted "the most important single palaeontological discovery" of Marsh's life.

In his more extended description of *Hesperornis regalis* in May 1872, Marsh considered it to be related to the Great Northern Diver *(Gavia immer)*, a reference that was later com-

pletely abandoned, as a result of the discovery, by T. H. Russell of the Yale party, of a nearly perfect skeleton of the same bird, including parts of the head and teeth—"an ample reward for the hardships and danger we incurred." When the great Cretaceous diver had been cleaned of all the adhering chalk, it was seen to be a bird 6 feet long and larger than any other known aquatic form, fossil or living. "The maxillary bones are massive," Marsh wrote, "and have throughout their length a deep inferior groove, which was thickly set with sharp, pointed teeth."

These astonishing discoveries were brought into even greater prominence in 1880, when Marsh produced his first monograph, a *magnum opus* entitled *Odontornithes: A Monograph on the Extinct Toothed Birds of North America,* a publication which was said by Henry Woodward, the English paleontologist, to surpass "any which have already appeared devoted to paleontology." This book, a sumptuous royal quarto with 201 pages of text, 40 woodcuts, and 34 lithographic plates, was published as one of the reports of the United States Geological Exploration of the Fortieth Parallel, under the direction of Clarence King, and also as Memoir 1 of the Peabody Museum of Natural History. The text was a most detailed description, bone by bone, of nearly entire skeletons of five species of toothed birds: *Hesperornis regalis, H. crassipes, Ichthyornis dispar, I. victor* (the two last named have curious biconcave vertebrae), and *Apatornis celer.* These descriptions were based upon about fifty different individuals of *Hesperornis* and seventy-seven of *Ichthyornis*—testimony to the care and patience with which Marsh's collectors combed the Kansas chalk for this rare material. "Never before," said Sir Archibald Geikie in his review of the monograph, "has it been possible, we believe, to reconstruct so perfectly so ancient an organism." The plates, which include a full-size restoration of *Ichthyornis* and one of *Hesperornis* half-size, were marvels of reproduction, "combined [with] an artistic finish which has made each plate a kind of finished picture." In defense of such elaborate plates, Marsh says in another paper (1885) that his aim was

"to do full justice to the ample material . . . and where possible, to make the illustrations tell the main story to anatomists. The text of such a

Memoir may soon lose its interest, and belong to the past, but good figures are of permanent value"

Aside from the above monograph, Marsh published fifteen pamphlets and notes on fossil birds between 1870 and 1880, to which he added but eight short articles during the next twenty years. In these twenty-four publications he described 1 new subclass of fossil birds, 2 new orders, 16 new genera (2 are synonyms: *Colonosaurus=Ichthyornis*, and *Lestornis=Hesperornis*), and 43 new species (3 are synonyms, 1 lost to Cope). Of these species, 1 occurs in the Jurassic *(Laopteryx priscus)*, 15 in the Cretaceous (12 in the Niobrara, 1 in the Claggett, 2 in the Lance), and 24 in the Cenozoic.

In *Odontornithes*, Marsh says: "The Struthious characters seen in *Hesperornis*, should probably be regarded as evidence of real affinity, and in this case *Hesperornis* would be essentially a carnivorous, swimming Ostrich." This conclusion "did not meet with general acceptance . . . and before long the Ratite affinities of *Hesperornis* were seldom alluded to in scientific literature." When Williston in 1896 described a specimen of *Hesperornis* with some of the feathers in place, Marsh commented that "these feathers are the typical plumage of an Ostrich," and rejoiced that this find proved "beyond dispute" that the nearest affinities of the Odontornithes were with the Ratitae. However, ornithologists of the present day still see no genetic connections here.

In his Nashville address, Marsh stated:

> "It is now generally admitted, by biologists who made a study of the vertebrates, that birds have come down to us through the Dinosaurs. . . . The case amounts almost to a demonstration, if we compare, with Dinosaurs, their contemporaries, the Mesozoic birds. The classes of Birds and Reptiles, as now living, are separated by a gulf so profound that a few years since it was cited by the opponents of evolution as the most important break in the animal series, and one which that doctrine could not bridge over. Since then, as Huxley has clearly shown, this gap has been virtually filled by the discovery of bird-like reptiles and reptilian birds. *Compsognathus* [once thought to be a bird but shown by Gegenbaur, Huxley, and Marsh to be a dinosaur], and *Archaeopteryx* ["the most reptilian of birds"] of the Old World and *Ichthyornis* and *Hesperornis* of the New, are

the stepping stones by which the evolutionist of today leads the doubting brother across the shallow remnant of the gulf once thought impassible."

At present, systematists would say that there is not much else besides feathers to distinguish birds from reptiles.

Marsh in 1880 regarded the Odontornithes, or birds with teeth, as a subclass, which he divided into three orders: (1) Odontocolcae, for the *Hesperornis* type; (2) Odontotormae, for the *Ichthyornis* type (in modern classification these are of the order Carinatae); and (3) Saururae of Haeckel, for *Archaeopteryx*. That these oldest types of true birds

"should differ so widely from each other points unmistakably to a great antiquity for the class . . . but the reptilian characters they possess are convergent toward a more generalized type. No Triassic birds are known. . . . [When they are found], if we may judge from Jurassic Mammals and Reptiles, the next classes above and below Birds, the avian forms of that period would still be birds, although with even stronger reptilian features. For the primal forms of the bird-type, we must evidently look to the Paleozoic; and in the rich land fauna from American Permian we may yet hope to find the remains of both Birds and Mammals."

This hope has not yet been realized.

Lucas (in Zittel, 1902 ed.) holds that the birds are "descended without question from reptiles, their affinities with that class are so intimate that Huxley included them both under the common designation of Sauropsida." He objects, however, and correctly, to this merging of birds with reptiles.

Alexander Wetmore, in his *Systematic Classification for the Birds of the World, Revised and Emended* (1934), agrees with Marsh's views as to the systematic relations between the Mesozoic birds and the more recent ones. He refers the Jurassic reptilian birds to the subclass Archaeornithes (ancestral birds), while placing the Cretaceous toothed birds in the subclass Neornithes (true birds), in the superorder Odontognathae (New World toothed birds), and in the orders Hesperornithiformes (*Hesperornis* and *Hargeria*) and Ichthyornithiformes (*Ichthyornis*). While this places them near the ostriches (superorder Palaeognathae or struthious birds) among recent birds, they are not considered as closely allied to that group.

Doctor Wetmore, after kindly reading the above section, made the following comment:

"Marsh's discovery of the toothed birds in the fossil deposits of Kansas though made at so early a day still ranks as one of the outstanding discoveries in palaeornithology in North America. And to his original studies little that is new has been added, except to refute Marsh's belief that these Cretaceous species were closely allied to the living ostriches. In point of fact the toothed birds seem set apart by themselves from all living forms

"In view of the very definite specialization of *Hesperornis* and its allies for an aquatic existence, and of *Ichthyornis* for life in the air it seems strange that no cursorial type has yet been discovered in Cretaceous deposits, though birds of this kind are found well developed in the Tertiary."

Mammals in General

Originally, Marsh intended to devote most of his time to the study of fossil mammals, but when he began to get the well preserved skeletons of gigantic dinosaurs, he became more and more overwhelmed by the extraordinary evolution shown in this phylum of reptiles, of which very little was known to paleontologists in the seventies. Accordingly, his projected monographs on the Mesozoic mammals, the horses, and the brontotheres failed to materialize, although the handsome one on the Dinocerata appeared in 1885 (author's edition).

Marsh treats of fossil mammals in eighty-five different publications, and his most productive years for describing them were the seventies. In these papers he describes as new 255 species and 120 genera; 38 of the species and 43 of the genera are known to be synonyms. Their variety is so great and their history is so intricate that of most of them little can be said in this memoir. In the list following, these mammals are grouped by orders, and those marked with an asterisk will be presented in more detail. Figures in parentheses indicate synonyms.

Subclass Allotheria. Mesozoic mammals.
    Order Multituberculata.
    Order Triconodonta.
Subclass Eutheria. Viviparous mammals.
    Infraclass Pantotheria.
        Order Symmetrodonta.

Infraclass Didelphia. Non-placental mammals.
  Order Marsupialia, opossums, kangaroos, etc.
  Of the above orders, Marsh described:
    *Late Jurassic mammals, 15 genera (9) and 25 species (6).
    *Late Cretaceous mammals, 19 genera (10) and 37 species (21).

Infraclass Monodelphia. Placental mammals. All of Cenozoic time. Has the following orders:
- Insectivora, 12 genera (6) and 18 species (3).
- Tillodontia Marsh, 1 genus and 4 species.
- Taeniodonta, of uncertain systematic relationship, 2 genera and 2 species.
- Cheiroptera. None of the "bats" described by Marsh belong in this order.
- Xenarthra, ground sloths or Gravigrada, 1 genus and 2 species.
- Rodentia, 5 genera (2) and 11 species (1).
- Carnivora, 11 genera (3) and 17 species (1).
- Primates, 5 genera (3) and 10 species (1). Five species are lemurs and five are tarsioids, the first of these primitive primates to be found in America.
- Condylarthra, primitive ungulates, 2 genera (1) and 3 species (1).
- *Amblypoda, short-footed ungulates, 3 genera and 23 species of *dinoceres.
- Coryphodontia, 1 species.
- Sirenia, sea cows, 1 genus and 1 species.
- Perissodactyla, hoofed animals with an odd number of toes, including the *horses with 8 genera (2) and 19 species (1); the *brontotheres with 13 genera (4) and 21 species (5); the tapirs with 2 genera and 3 species; and the rhinoceroses with 5 genera and 14 species.
- Ancylopoda, clawed animals with an odd number of toes, 1 genus and 3 species.
- Artiodactyla, cloven-hoofed animals with an even number of toes, including oreodonts, swine-like mammals, camels, deer, antelopes, etc., 21 genera (4) and 49 species (1).

Mesozoic Mammals

From the phylogenetic viewpoint, the *Jurassic mammals* are probably the most significant of the whole class Mammalia, but unfortunately these fossils are amongst the greatest rarities, nearly all the known specimens being fragmentary jaws and isolated teeth. Even so, as G. G. Simpson said in 1928, "one is at least dealing for the most part with jaws with their included teeth and direct comparisons between the established genera are possible in most cases".

The first Mesozoic mammal jaw was found in England in 1764, but it was not recognized as such until 1824, when the great anatomist, Cuvier, saw it and pronounced it mammalian. In 1871 it was named *Amphilestes broderipii* by Owen. Professor Marsh, from his European studies, was fully aware of the desirability of obtaining more of these early mammals, and he was constantly urging his field men to be on the watch for small fossils. Due to this prodding, one of his best collectors, W. H. Reed, excavating for dinosaurs at Como Bluff, Wyoming, in 1878, found a good lower jaw of a mammal about the size of a weasel. Marsh named this find *Dryolestes priscus*, and added that it represented a marsupial "allied to the existing opossums". Late in 1879, Marsh named more of these "medieval" mammals, saying that they show "such a resemblance to known types from the Purbeck of England, that some connection between the two faunae is clearly implied." Further study convinced him that these mammals

"cannot be satisfactorily placed in any of the present orders. This appears to be equally true of the European forms . . . With the exception of a very few aberrant forms, the known Mesozoic mammals may be placed in a single order, which may appropriately be named Pantotheria."

This order is still recognized by some systematists, and Marsh's order Allotheria is now raised to a subclass.

These finds led to a more systematic search for mammals, notably in the famous Quarry 9 at Como, from which, thanks to a year's careful and persistent search, nearly all the more than 400-500 separate specimens of American Jurassic mammals have come. The cost involved in getting these tiny fossils makes them worth more than their weight in gold, and they are among the great treasures of the museums at Yale and at Washington.

Carrying out Marsh's original intention, all the Mesozoic mammal material thus collected has now been elaborately monographed by George G. Simpson (1928), who finds 44 Jurassic species in 23 genera. However, even our present knowledge of these early mammals, garnered over more than a century of endeavor by many paleontologists, represents, according to Simpson, only "lights in the vast darkness of the Age of Reptiles—and very dim lights most of them are".

No authentic *Cretaceous mammals* appear to have been found anywhere until 1882, when isolated teeth of such were discovered by J. L. Wortman in the Laramie formation of Wyoming. It was, however, not until 1889 that they began to be found in any quantity, Hatcher writing Marsh on May 20 that he was sending to New Haven by registered mail "a package containing some 4 or 5 species of Laramie mammals. . . I hope you will be pleased and will not despise them because they are few in number. They are by no means abundant, the few I send you requiring several days careful search." Marsh was pleased indeed, so much so that on June 8 he telegraphed Hatcher to stop work on ceratopsian skeletons and go after mammals entirely, for which he then had four different localities. Within the next four years, Hatcher, assisted by C. E. Beecher and other collectors, sent to Yale about 5000 teeth and some jaws and skeletal parts. As early as July 1889 Marsh had a paper in print describing 12 genera and 18 species of these Cretaceous mammals, and announcing that he had in preparation for the United States Geological Survey a memoir on this "rich mammalian fauna". In this paper he gives credit to Hatcher for the discovery of "material for a new chapter in paleontology".

Simpson's study finds that these Cretaceous mammals are still very inadequately known, because of their usual isolated occurrence as teeth "which cannot be associated into natural genera in the majority of cases . . . the characters of two consecutive teeth of a single genus cannot be determined in many instances." Marsh was aware of the necessarily artificial nature of his classification of these Cretaceous mammals; as Simpson says, Marsh had to resort to an analytical basis, giving names "not to distinct animals but to different types of teeth. Under the circumstances there was much to be said for this procedure." Even yet the time is not at hand for a synthesis of these teeth into genera and species based on entire animals, and Simpson concludes that "a revision, in the strictest sense, is impossible".

The Curious Dinocerata

The relatively abundant short-footed amblypods for which Marsh erected the suborder Dinocerata originated in Wyoming

during the late Paleocene and died out at the close of the Eocene epoch, being the most striking and characteristic animals of middle Eocene (Bridger) time. They are also known in Mongolia, whither they migrated from America. Some of the larger forms, standing 6-7 feet high at the shoulders, were elephant-like in bodily build, but they had no trunks and their curious heads were wholly unlike those of the proboscideans in that they bore three pairs of horns; and, in the males at least, the upper canine teeth were drawn out into long recurved saber-like tusks that must have been terrible weapons, although the manner of their use is unknown. The brain was exceedingly small.

While Leidy was the first to discover bones of the Dinocerata, his material was very fragmentary. To Marsh belongs, as Wortman said in 1899, "the credit of the final determination of their structure and affinities; he classified them in a separate and distinct order, Dinocerata, a name which has been very widely adopted by naturalists."

In September 1870, Professor Marsh, with a large party of Yale students, explored the Green River basin of western Wyoming under military escort. Here they found a large "boneyard", in which mammal remains were the most abundant fossils. Among these was a partial skeleton which Marsh the following year referred doubtfully to Leidy's genus *Titanotherium,* calling the species *T. ? anceps.* He returned a number of times to this basin, and to other nearly as rich ones in the Green River country, and brought back many more specimens of this group, which he named Dinocerata in 1873, and of which he finally had more than two hundred individuals, including some twenty skulls in good condition—striking testimony to the tenacity and thoroughness so characteristic of Marsh as a collector.

Between 1872 and 1885, Marsh issued no fewer than thirty-four papers treating of the Dinocerata, the series culminating in the quarto volume entitled *Dinocerata, a Monograph of an Extinct Order of Gigantic Mammals,* which, illustrated by 200 woodcuts and 56 lithographic plates, ranks among his best studies. This volume describes 3 of Marsh's genera, *Dinoceras, Tinoceras,* and *Laoceras* (subgenus), and 1 of Leidy's, *Uinta-*

therium, the four together having 23 species named by Marsh and 6 named by other authors (Cope 3, Osborn 2, Leidy 1).

It was the naming of these mammals that led in large part to the intense rivalry between Cope and Marsh, into which the older Leidy—a man "insensible to and unaffected by the ordinary passions of ambition or rivalry"—was unfortunately drawn.

While a student in Germany, Marsh had met Cope, and previous to 1870 they had exchanged friendly letters. Shortly after Marsh's return, he called on Cope at Haddonfield, New Jersey, collected fossils with him, and purchased some of his material. Marsh's expedition to the West in 1870, however, and its great success in securing vertebrate fossils, as reflected in his papers of 1871 describing 4 new genera and 27 new species, showed the Philadelphia group, represented by Leidy, Cope, and Hayden, that here was a very real competitor.

Hayden had begun exploring on his own account in the Missouri River country as early as 1854; in 1856 he joined Lieut. G. K. Warren's survey, and in 1859 that of Capt. W. F. Raynolds. In 1867, he was called on to organize the United States Geological Survey of the Territories, which, at the time of its formation, was one of four separate national surveys in the Rocky Mountain country. All the vertebrate fossils collected by Hayden had been turned over for study to Leidy and later to Cope, and up to and including 1870 the former had described from the West slightly more than 100 new species. After the early seventies, Leidy dropped more and more out of the field of western vertebrate paleontology, but Cope in 1872 published thirty-four papers and notes, a strong indication of his intention to remain there.

When Congress abolished the four independent surveys in 1879, and called for a single new organization, the United States Geological Survey, to be headed by Clarence King—a reorganization with which Marsh had considerable to do—Cope and Hayden found themselves supplanted in their work for the federal government.

Returning to the battleground of the Dinocerata, Marsh published thirty-five papers on this group between 1871 and 1884, and Cope at least twenty-nine. Seven of Cope's papers

describing new genera and species were dated 1872; Marsh doubted this date and he set to work to find out the actual dates of issue, presenting his findings in ten different papers appearing during 1873. As a result of this tangle of conflicting dates, the taxonomy of the Dinocerata is even yet not settled, nor will it be until some judicially minded vertebrate paleontologist, fully conversant with the International Rules of Nomenclature, studies all the great mass of material in the various museums.

The Great Brontotheres

On the third Yale expedition to the West, in 1872, two members of the party, H. B. Sargent and J. W. Griswold, found remains of a huge new mammal, to which Marsh in the following year gave the interesting name *Brontotherium gigas*, the "great thunder beast." Marsh showed this striking new type to be a true perissodactyl, and, according to Osborn, "was able in a very few words to throw a flood of light upon the characters of the skeleton." These animals are often called titanotheres, but since the generic name *Titanotherium* (of Leidy) no longer stands, it would seem that for the name of the group we should fall back upon Marsh's term brontotheres; this is also W. B. Scott's conclusion (1937). These creatures once roamed in great herds over what is now the Great Plains of eastern Colorado, Wyoming, Dakota, and Nebraska. Their brains were no larger than a man's fist even in the largest of the group, which attained almost the bulk of an elephant. As Scott says, these great beasts "were even more dull and stupid than are modern rhinoceroses."

Although elephantine in bulk, the brontotheres were less heavily built proportionally, and stood somewhat higher. The head was saddle-shaped, with a blunt horn on either side of the nose. The first of the group were comparatively low of stature, and their "horns" were small knobs, well back of the eyes. With the passage of geologic time, the horns grew longer and the animals larger, so that the skull became a yard long and the horns a foot high and on the very end of the nose. These animals arose early in the Eocene of North America, and died out in the Oligocene, a geologic interval of about ten million years. Recently they have been discovered in the Eocene and

Oligocene of Mongolia. Their genetic evolution took place along four main stems, but counting all the side branches, Osborn later indicated not less than eight phyletic lines, with 23 genera, 8 of which were named by Marsh.

Between 1873 and 1891, Marsh published thirteen papers on these brontotheres. In 1889, he presented a restoration of *Brontops robustus*, pointing out that it represented the largest animal of its time. Bones of this animal were first found in 1874 by H. C. Clifford, south of the Black Hills near Chadron, Nebraska, not far from the White River, but not until 1886 were most of its parts finally recovered, and the left hind leg is still missing; the mounted skeleton is one of the greatest treasures in the Peabody Museum.

In the early 1880's Marsh planned a large and well illustrated monograph on the brontotheres, and for it made sixty lithographic plates, but at the time of his death in 1899 he had not even begun to prepare the manuscript. The United States Geological Survey in 1900 transferred the task of writing this monograph to H. F. Osborn, but he was not able to finish it until 1919, and another ten years passed before the two handsome volumes appeared under the title *The Titanotheres of Ancient Wyoming, Dakota, and Nebraska.* They form the most far-reaching work on a single group of vertebrate fossils ever published, and it is pleasant to read in them that Marsh

"made the largest and most valuable contributions to our knowledge of this family and of its evolution. He planned the monumental field work of John Bell Hatcher, by which the great collection for the United States National Museum was made [which has more than 158 skulls and jaws] and he supervised the preparation of the sixty lithographic plates, which are here reproduced."

Increase of Intelligence in Geologic Time

From 1870 on, whenever Marsh had sufficient material, it was his rule to have his preparators section the rear end of fossil skulls and clean out all the rock or crystalline material in the brain cavity. Into this cavity was then poured warm gelatine, which, because of its marked pliability, could be easily pulled out of the cavity, when cold, without tearing off any of the projecting parts. From this gelatine cast a more permanent

mold would be made that permitted the taking of other replicas.

By 1874, he had brain casts of many Cenozoic mammals, and these enabled him to make a first attempt at a generalization regarding brain growth in geologic time. On the evening of June 17, he presented his conclusions before the Connecticut Academy of Arts and Sciences in New Haven. The Eocene mammals, he said,

"all appear to have had small brains, and in some of them the brain cavity was hardly more capacious than in the higher reptiles. The largest Eocene mammals are the *Dinocerata,* which were but little inferior to the elephant in bulk. In *Dinoceras* . . . the brain cavity is not more than one-eighth the average size of that in existing Rhinoceroses. . . . The gigantic mammals of the American Miocene [=Oligocene] are the *Brontotheridae,* which equalled the *Dinocerata* in size. In *Brontotherium* Marsh . . . the brain cavity is . . . about the size of the brain in the Indian *Rhinoceros.* In the Pliocene strata of the West, a species of *Mastodon* is the largest mammal, and although but little superior in absolute size to *Brontotherium,* it had a very much larger brain, but not equal to that of existing Proboscidians. The Tapiroid ungulates of the Eocene had small brain cavities, much smaller than their allies, the Miocene *Rhinocerotidae* The Pliocene representatives of the latter group had well developed brains, but proportionally smaller than living species. A similar progression in brain capacity seems to be well marked in the equine mammals."

In 1876, Marsh briefly recapitulated his knowledge as follows:

"First, all Tertiary mammals had small brains; *second,* there was a gradual increase in the size of the brain during this period; *third,* this increase was mainly confined to the cerebral hemispheres, or higher portion of the brain; *fourth,* in some groups, the convolutions of the brain have gradually become more complicated; *fifth,* in some, the cerebellum and olfactory lobes have been diminished in size."

These statements he repeated in his Nashville address the following year, adding:

"In the long struggle for existence during Tertiary time, the big brains won, then as now; and the increasing power thus gained rendered useless many structures inherited from primitive ancestors, but no longer adapted to new conditions."

In his Presidential Address at Saratoga in 1879, he went still further, saying:

"More recent researches render it probable that the same general law of brain-growth holds good for birds and reptiles from the Mesozoic to the

present time. The Cretaceous birds, that have been investigated with reference to this point, had brains only about one-third as large in proportion as those nearest allied among living species. The Dinosaurs from our Western Jurassic follow the same law, and had brain cavities vastly smaller than any existing reptiles."

To the five conclusions regarding brain growth stated in 1876, and later presented as an "outline of a general law of brain growth," Marsh made two additions in the Dinocerata monograph (1885), as follows:

6. "The brain of a mammal belonging to a vigorous race, fitted for a long survival, is larger than the average brain of that period in the same group."

7. "The brain of a mammal of a declining race is smaller than the average of its contemporaries of the same group."

HONORS

When one considers that in Professor Marsh's time, honors and other evidences of distinguished achievement were not as numerous as they are now, those that he received make an impressive list.

On the academic side, his record includes the class valedictory at Andover, a High Orations stand at graduation from Yale, a Berkeley Scholarship, and election to Phi Beta Kappa; he received an honorary Ph.D. from Heidelberg University at its 500th anniversary in 1886, and Harvard's doctorate of laws in the same year, at its 250th anniversary.

President of the American Association for the Advancement of Science in 1878, he was vice-president of the National Academy of Sciences from 1878 to 1883, and its president from 1883 to 1895. He was vertebrate paleontologist of the United States Geological Survey from 1882 to 1899, and honorary curator of vertebrate paleontology in the United States National Museum from 1887 until 1899.

From the Geological Society of London, of which he was elected a Fellow in 1863 and a Foreign Member in 1898, he received the Bigsby Medal in 1877; and twenty years later he received Vertebrate Paleontology's *cordon bleu,* the Cuvier Prize from the Institute de France, becoming Correspondent of the French Academy the next year.

He was a member or an honorary member of forty-one scientific societies or academies, and six of a non-scientific nature, distributed as follows: United States, 26; England, 4; Belgium, 4; Germany, 4; France, 2; and one each in Canada, Mexico, Argentina, Ireland, Denmark, Italy, and Russia.

His epitaph, written by his lifelong mentor, Professor Brush, reads: "To Yale he gave his services, his collections and his estate."

"Here are they to whom, from the depths of space, were whispered in the night watches its long hidden secrets. There, too, are those who, in the silence of the laboratory, rejoiced in the fertile marriage of the elements, or they who, like confessors, heard from dead bones or rock or flower the immeasurable history of the silent ages of earth."

S. Weir Mitchell,
At the Jubilee banquet of the National Academy of Sciences.

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OF

Othniel Charles Marsh

1831–1899

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Walter Jones

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OF

WALTER (JENNINGS) JONES

1865–1935

BY

WILLIAM MANSFIELD CLARK

PRESENTED TO THE ACADEMY AT THE ANNUAL MEETING, 1938

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Those who knew Walter Jones use thread of gold for the warp of the tapestry that they weave on the loom of memory. They float the golden warp for the figures of the eloquent teacher, the keen investigator and the kind friend. Because the unique personality made extraordinary impressions, every weaver uses highly colored, homespun yarn for the wefts of his tapestry.

While we may draw the figure of the scientist from the written record, we have little more than recollections with which to sketch the man. Walter Jones seems never to have thought of retreat into the realms of his own recollections. He left among his effects, as records of his career, only an incomplete set of reprints, a list thereof, and three diplomas. Indeed, this adventurous warrior burned every bridge as he left it behind. It was his habit to tear up a letter while he still mused upon the content. He wrote few extensive letters and when at long last he had a secretary he was known to go to the typist of another department that he might dodge the carbon copy that his secretary would have filed. When he retired he thrust some of his own choice possessions into the hands of assistants who had carried on faithfully during his illness.

Fate conspired in this plotting of obscurity. At every place in Walter Jones's career circumstances or accidents prevented or destroyed the written record of his ordinary activities.

Since living the moment at its full was characteristic of the man, his biographer must not complain that Walter Jones left little with which to document events or to verify reports of his ideas. And yet we wish there were a substantial body of documentary evidence concerning the thoughts that were not constrained by the merciless rules of scientific publication. It would be interesting to have enough to discover some relation between the attitude of challenge that Jones bore in daily life and the part he played in science. It might be inspiring, could we discover the aspirations of the whole man in the eloquence of his lectures. And, I suspect that there would disappear misunder-

standing of tales that have become legendary, were it possible to resolve by documentary evidence some of the enigmatical parts of this character.

I venture to suggest that a good deal of the mischief in which he gaily indulged was his delicious way of telling people not to take too seriously the intensity of his feeling in matters that he had to take seriously. Certainly, the torrent of Walter Jones's conversation carried none of the wreckage of a reformer. It was not a muddy freshet; it roared perennially in a cañon cut deep in the stratified thought of his time.

So much conjecture would have to enter serious attempts to resolve some parts of this character that a biographer had best hold himself in check. Nevertheless, it seems permissible to present certain facts of the life in suggestive juxtaposition and with such comments as will be illuminating while recognized as tinged with the biographer's suppositions.

Walter Jones's forebears were Marylanders through several generations; the paternal ancestors being dwellers on the Eastern Shore of the Chesapeake Bay country, and the maternal on the Western Shore. For finding or checking the following information about them I am indebted to Miss Harriet Perkins Marine.

The fractionation of Walter's genes through the straight male line begins with Levin Jones [b. 1799, d. 1878] the father, and thence through Levin Jones the grandfather to William Jones, a first settler from Wales. Levin Jones (the son of William) married Nancy Jones [b. 1769] who was the daughter of Roger Jones and Elizabeth, his wife. Roger Jones was an ensign and captain in the War of 1776 and his brother Thomas was a colonel who, by family tradition, was an aide to General Washington.

The most picturesque of Walter's collateral ancestors who fought in the War of Independence was Colonel John Jones (son of Colonel Thomas). One of his most famous exploits was performed when a group of three British transports with troops put into the Little Choptank seeking refuge from a storm. The story is that the Colonel summoned his neighbors who took positions behind ice banked high on the shore by the storm. From these natural ramparts at daybreak they peppered the

vessels with shot and slugs from their duck and squirrel guns. Perhaps it was "36 hours" of peppering or perhaps it was an argumentative skill, so dominant as to turn up again in great-grandnephew Walter, that finally induced the British captain to surrender when once he had lost the first point of the argument by raising the white flag.

Through his great-grandmother, Nancy Jones, Walter Jones was fifth in descent from Justice John Jones [b. 1699, d. 1774] and his first wife, Sarah Woolford [b. about 1704, m. about 1726, d. after 1775]; sixth in descent from the first settler Thomas Jones of Somerset-Dorchester Counties and his wife, Martha Davis [b. 1670, m. about 1688], daughter of William Davis, who came to Maryland from Virginia in 1664. Sarah Woolford, the daughter of Colonel Roger Woolford [b. 1670, d. 1730] and Elizabeth Ennalls [b. about 1668], descended from the following first settlers of the Province of Maryland: her grandfathers, Roger Woolford [b. in Wales, d. 1701] and Bartholomew Ennalls [d. 1688] and her great grandfather, Levin Denwood [b. 1602, d. after 1665].

Thence came to Walter's family its pride in having descended from Welsh ancestry and from early settlers of Maryland and Virginia; a pride to which Walter was indifferent.

Several of the men on the male and distaff sides of this part of the ancestral tree were holders of large lands in Maryland and Virginia and active in the services of their local governments.

Levin Jones, Walter's father, left the Eastern Shore and settled in Baltimore as a ship chandler with his business at the Light Street Wharf. He had gained the sobriquet *Captain* by way of owning a small fleet of vessels. He was a substantial business man and owned considerable property. It seems appropriate to mention his nervous habit of shuffling coins while he spoke, for Walter's similar habit of tearing letters while he mused had consequences that we regret. The father died when Walter was thirteen years of age. His widow, who was twenty-two years younger, survived him twenty-eight years.

Walter's mother, Zeanette Jane Bohen [b. 1821, m. 1840, d. 1906] was the daughter of James Bohen [b. 1797, d. 1840]

and Sarah Ann West [b. about 1800, m. 1820, d. prior to her husband]. James Bohen traced his ancestry [Bohn] to an ancient English family of noble estate. Sarah Ann West was the daughter of Benjamin [Ben] West and Annie Spencer and was related to the Wests, Spencers, Hopkinses and other prominent families of Prince George's and Anne Arundel Counties. Walter's mother, like her husband, was very active in the affairs of the Methodist Church in Baltimore. She was one of the founders of The Nursery and Child's Hospital and of The Home of the Aged of the Methodist Episcopal Church. She was one of the vice-presidents of the latter institution at the time of her death.

Walter Jones was born at Baltimore in the City's period of distress. The date was Friday, April 28, 1865. This was two weeks after the death of President Lincoln became the climax of the Civil War. That war's effect on Maryland is described by Mr. Gerald Johnson.[1]

"Driven by internal dissension, drawn by affection in one direction and by interest in another, suspected and reviled by both sides, exposed to all the horrors of war without enjoying its fierce exaltation, sharing the dangers, the losses and the woes of both North and South, but never with any part in the triumphs of either, it [*Maryland*] was trampled under the feet of both contestants and emerged beaten and broken."

Walter Jones was too original to have imitated those of his generation who ruminated what their parents had taken in, but he must have lived on the war's aftermath. Other men have said that this storm-spoiled fodder sickened them; emotionally Walter Jones had a weak stomach. In a letter concerning the World War the ageing man did not write of issues, or of defeat, or of history; he wrote of confusion. Throughout life Walter Jones frequently gave the impression of one who felt the world to be out of joint.

The Civil War also occasioned the first of the obliterations of the records pertaining to the fateful Walter. The churches at that time were lax in keeping records. Several hid their books. Diligent search therein has failed to reveal any church

[1] *The Sun Papers of Baltimore*, by Gerald W. Johnson, Frank R. Kent, H. L. Mencken and Hamilton Owens.

record of Walter's birth and baptism. Indeed, it has failed to bring to light any record concerning the birth or baptism.

Could a record be found it might illuminate a curious matter. Walter was given the middle name *Jennings* for a physician and friend of the family. *Jennings* was used on his wedding invitation and in one autobiographical sketch. The initial *J* appears on the paper he published with Stone. But it is alleged that later he dropped the middle name because he had heard that it was not pronounced at his baptism. Consequently few of his later friends ever heard of *Jennings*. On being told of the middle name one of his friends declared testily, "His name was Walter Jones!" So be it. His work clearly distinguishes him from that brilliant and eccentric barrister of the same name who won a place in the history of Virginia.

The family life that was to be Walter's lot was that of well-to-do people. Both the father and mother owned considerable property. The family life seems also to have been that of people moderately well educated according to the common standards of the time. Thence must have come naturally to the boy those simpler graces encompassed in the larger meaning of *grammar*. Schooling alone hardly could have given the mature man his unerring ease of expression, although it may have polished that handwriting which recipients of letters have likened to engravings.* It is evident that the boy was familiar with the better known classics of English literature and that his love for music had early nourishment. On the other hand, there seems to have been no close association of the family with any of those intellectual vocations or avocations that might have fixed a comfortably conventional attitude toward academic ideas. On the contrary, the ideas current in Walter's familiar environment were sufficiently undomesticated to have made him unconcernedly used to the appellative *eccentric*.

The evolution of the family has established that the youngest of a large family has the inalienable right to be mischievous. As the thirteenth child, Walter was placed, tentatively, in a very favorable position; as the last child his opportunity was assured.

* The tremulous execution of the signature under the portrait is of late date.

The temperament to seize it was determined by the genetic dice that turned up a strange group of characteristics—among them volatility. When Emerson remarked, "We boil at different degrees", it was not incumbent upon the essayist to add what his experiences as a chemist and as a moralist had taught, namely, that a boiling point is a function of restraining pressure. This principle seemed inapplicable to the spirit of Walter Jones. No occasion was so oppressed with dignity as to suppress his sense of humor; no personage so high that he would not dare to banter. In the light byplay of laboratory life Walter Jones was frequently up to something. His grandest opportunities for mischief arose from his conversational ability. He was a brilliant conversationalist and yet so forthright that he said of himself, "When I talk the loudest, I know the least". He could be frank to the verge of offense and yet he had a student say of him, "One always knew where one stood with Walter Jones and that was a great comfort". Of course, so forthright, frank and vigorous an ideologist would develop a reputation of a sort. The sort Walter Jones himself would indicate by twirling his finger about his head. Here was his grand opportunity. On occasion, this creature of opinion would be put into action to make the creators dance. On occasion, the creature was made a convenience. An undesired applicant for laboratory privileges was told, "You won't want to work here. I'm crazy."

The house where Walter was born was located on South Sharp Street, now called Hopkins Place, between Lombard and Redwood Streets.

There may be a significance more than geographical in the fact that a brother has described the particular location of the birthplace by its position relative to a Baptist church, an Episcopal church and a house "where a venerable Hicsite Quaker lived." Both parents were devout Methodists in a city rich in Roman Catholic traditions. From the fact that the mature man seldom, if ever, was tripped on a catch-question regarding the Bible, it is fair to assume that the early catechistic training was intense. Indeed, there is testimony of long Sunday school attendance and teaching. Some near relatives were addicted to strict religious observances. Both the father and the mother were promi-

nent in the affairs of their church and had the reputation of being generous to a fault in the giving of their services and their fortune to the work of the church

In recalling this familiar background, it is important to realize that the boy was imbued with a faith which was part of a great movement, sometimes called the romantic rebellion against an earlier "age of reason". The boy was born to a generation of followers who found themselves resisting the devastation of a new age of reason. Among the many issues "Darwinism" was but one and "The Higher Criticism" one of the others. In its own peculiar way each of several issues seemed to concern something worth fighting for. The time was to come when the significance of these issues faded, either because some people had tiptoed around them or because others had risen to new enlightenment without succumbing to that evil aspect of tolerance, indifference. But let no one now, in hauteur, point merely to straight-laced practices of the stock from which Walter Jones came or attach *superficial* implications to the subject matter of Walter Jones's discussions. In all of his discussions there must have been more poignancy than this generation can feel.

There need be no hesitation in writing that Walter Jones made much of theological discussions. Whatever may have been his religious feeling at any time, his theological views were not so precious as to shield them from courteous biography. He proclaimed his views to confreres, to agents for chemical supplies, to his bank clerk, and to his friends among the clergy. Probably no one ever knew his deeper musings and probably no label would have been acceptable, but it was made plain that he could not reconcile the tenets of theology and science. From much testimony of his attitude I select one bit that seems to jibe best with the whole. It is a straightforward story told by one who listened to a long series of conversations. This was during Jones's correspondence with an eminent physicist who had publicly declared his own reconciliation. It is reported that Jones maintained the highest respect for his correspondent and earnestly looked to him for a constructive contribution. But answers came that seemed to Jones evasive and to be carrying logic into the

fog of wishful thinking. Then he gave vent to one of those tirades that too often led people to say "he baited his victim." Rather it was that Walter Jones, facing in his way the greatest problem of his age, saw the devil and threw his inkwell.

It goes without saying that tirades on religious subjects and a good deal of provocative banter is a combination likely to lead to grave misunderstanding. Therefore it is fortunate that Walter Jones worked among men whose instincts gave a natural respect to his peculiar need for freedom of thought and freedom of expression. Without this perfectly natural respect Walter Jones could not have done the scientific work that he did. This man of complete independence could not have endured freedom at the cost of condescension. There was no need. That grand gentleman, John Abel, who held the control of Walter Jones's early academic career, always kept first things first. He respected Walter Jones's scientific ability and in this atmosphere Walter Jones won his own way and fame.

As a boy Walter was active in sports. He hunted and was a very good tennis player. He is remembered by a nephew, of similar age, as "one of the best fancy ice skaters in Baltimore, the envy of the younger ones as he performed on Sumalt's ice pond." No one seems to understand why he suddenly ceased to be active out of doors, but there can be no doubt that he maintained his interest in sports. He told Professor Abel that he "went with the football crowd at Purdue" and throughout his life at Johns Hopkins he was an ardent lacrosse fan and a vigorous critic of the players not only in their contests but also in their routine practice. Some early habits may have determined the carriage of the tall, lank professor who was physically active and gracefully so.

The young man played the piano well enough to afford entertainment. The mature man collected records of classical music and knew them well. In music Baltimore offered him opportunities that he seldom missed and once mentioned to a class in this manner: "The usual recitation is scheduled for four o'clock tomorrow. Those of you who are uncivilized do not know that the Boston Symphony Orchestra is to give a special concert at that hour and so you will report here promptly. *I* shall not be here!"

Walter's early education was obtained partly in small private schools of his neighborhood and partly in public schools. Whatever these may have contributed was enlivened by the devotion of his sister Annie who followed his studies keenly and who in later life declared that she had attained a college education through Walter's eyes. This personal touch may well have exerted its potentially great effect. Tutorial habit exercised with a loving pupil can fix the photographic image. It can develop the high lights. That Walter's image of a lesson was clear is attested by college mates who heard him recite. The mature professor expected the same of his pupils. They could laugh at his extravagance while feeling the significance of his exclamation to a forgetful student, "Even the Baltimore street car conductors know the amino acids!"

In 1879 Walter entered the City College of Baltimore for its five-year course. The College has no record except that he completed the course creditably in the spring of 1884.

The following fall, Walter Jones registered in the Collegiate Department of The Johns Hopkins University, stating, "I wish to take a course which will be principally mathematics and Latin. I also wish to study chemistry, French and German." He took the courses then known as Group IV with chemistry and physics predominating in the last two years. With his progress in course his grades improved until he became a high standing student and won a University Scholarship for the year 1888-9. Granted the B. A. degree in 1888, Walter passed into graduate work in chemistry, taking minors in mineralogy and geology. Work for his dissertation was done under Professor Ira Remsen. He was granted the degree of Ph.D. in June, 1891.

Since few of the contemporary students recall anything especially worthy of note regarding Walter Jones, it may be inferred that he then exhibited what was characteristic of him in later life—supreme independence in going his own way.

On September 1, 1891, there occurred in St. Paul's Church, Ocean Grove, the marriage of Walter Jennings Jones and Grace Crary Clarke. Miss Clarke was the daughter of the Rev. and Mrs. George Clarke of Ocean Grove, New Jersey. To this seaside resort Dr. and Mrs. Jones went frequently on vacations.

Soon after his marriage, Doctor Jones took his bride to Springfield, Ohio, where he had been engaged as Acting Professor of Natural Science in Wittenberg College. The engagement there was for one year during which the professor of that subject, A. F. Linn, returned to Johns Hopkins to complete some investigations under Remsen. As was customary in those "good old days", Professor Jones "offered courses in chemistry, mineralogy, zoology, and botany. It is possible that he may have offered a course in crystallography." [2]

His first appointment terminated, Dr. and Mrs. Jones returned to Baltimore. There, August 13, 1892, was born their only child, Marion Eleanor.[3]

One may read between the lines of the following, taken from a letter concerning The American Society of Biological Chemists.

"The meeting of the Society during Christmas recess has always been a matter of regret to me, for this is the time of year that I have been accustomed to devote to home affairs since I was a child and it is very difficult indeed to break away from such a custom."

His granddaughter, Charlotte, made Walter Jones her devoted playmate.

In September, 1892, Doctor Jones went to Purdue University as Professor of Analytical Chemistry. Winthrop E. Stone, Professor of Chemistry, had been made vice-president of Purdue and already was taking over a good deal of the administrative routine for which he soon was to assume full responsibility as president. Yet, when Jones arrived, Stone drew him into an investigation on which the two men reported. Jones later contributed an article from Purdue on a problem related to a subject then lively at Hopkins. Dean Enders informs me that the records of Purdue University contain nothing regarding Walter Jones other than that here given. That Jones finally became dissatisfied at Purdue he told Professor Abel. At any rate Jones returned to Baltimore without a job and again taking up work under Remsen was made a Fellow by Courtesy for the year 1895-6.

[2] Letter from Dean Shatzer.
[3] Marion married Gilbert A. Jarman of Baltimore.

The only enlightening information from Wittenberg and Purdue is that Walter Jones was then a vigorous teacher who used unique ways of stimulating student interest and who challenged students to questioning.

In March 1896 Professor John J. Abel took Jones to the medical school where he was appointed Assistant in Physiological Chemistry for the remainder of the academic year. There Walter Jones was to remain for the greater part of his career.

Since Walter Jones was to become the head of the Department of Physiological Chemistry in the Johns Hopkins School of Medicine, it is well to review briefly the early history of that department.

The original plan for the Medical Department [4] of The Johns Hopkins University included the teaching of chemistry to students of medicine under the auspices of the established Department of Chemistry. Indeed, the first *Announcement* lists the head of that department, Ira Remsen, as Professor of Chemistry in the roster of the Medical Faculty. Remsen held this title until he became the second president of the university in 1901. Remsen had been one of the committee to consider the establishment of the Medical Department and he continued on its Advisory Board, first as an elected member and finally *ex officio* as President.

With the opening of the school in 1893 the original plan of teaching preclinical subjects was modified as the exigencies demanded. The teaching of physiological chemistry was entrusted to the professional school under an arrangement stated as follows in the first *Announcement* of 1893. "The instruction in Physiological Chemistry will be for the present under the charge of Dr. John J. Abel, Professor of Pharmacology, with the aid of an assistant."

It is important to note that physiological chemistry was given a unique position in the new school. The original intention to have medical students trained under the guidance of the Depart-

[4] The name was changed during President Goodnow's administration to harmonize with *School of Hygiene and Public Health*, *School of Engineering*, etc., and is now *The School of Medicine, The Johns Hopkins University*.

ment of Chemistry was a reflection of the purpose to train medical students as university students. The actual, initial placement of physiological chemistry in the Department of Pharmacology broke an historical relation that had made the subject the foster child of physiology. The entrusting of the teaching to Professor Abel gave an especially noteworthy character to the whole affair.

Professor Abel had come, directly and indirectly, under the influence of several German investigators, who, although trained in the general field of medicine, had acquired an expert's knowledge of chemistry and who devoted it to fundamental work. This influence, and Professor Abel's own keen appreciation of chemistry, made him determined to give the science its full due. He wished to avoid the limitations of the purely "analytic school" and the subordinate place of chemistry suggested by the term "chemical physiology." Abel's attitude is reflected by Jones's specification for an assistant that he sought in 1925, nearly thirty years later

"We want a man who has a Ph.D. degree, who has teaching ability as well as research ability and who is well grounded in the fundamentals of chemistry. Of course, it is desirable that he should also have had a training in physiological chemistry and the biological sciences but he must be a chemist primarily."

While the burdens of organization and the financial catastrophe in the early years of the school inhibited the development of Abel's plans, he managed to create the plant out of which brilliant investigations were to blossom. It was at the beginning of this flowering that Walter Jones came.

When the school was opened in the autumn of 1893 the students, of course, had not reached pharmacology and Professor Abel, with his first assistant, Dr. Thomas B. Aldrich, could handle the introductory course in chemistry. Thereafter, Abel turned over the instruction in this subject to assistants. There was also a spatial separation of the work, chemistry being left for a time in the old Pathological Building and pharmacology going with anatomy to the newly built (1894) Women's Fund Memorial Building until chemistry and pharmacology were reassembled in the Physiology Building in 1898.

Thus, when Jones came to assist in the teaching of physiological chemistry, there was no autonomous department for that subject, although the subject was taught as a distinct discipline. At the same time the close relation between subjects taught under Abel's general supervision carried Jones into the teaching of toxicology.

The list of Jones's successive titles is instructive:

1896-8 Assistant in Physiological Chemistry.
1898-9 Assistant in Physiological Chemistry and Toxicology.
1899-1902 Associate in Physiological Chemistry and Toxicology.
1902-8 Associate Professor of Physiological Chemistry and Toxicology.

1908 (Department of Physiological Chemistry established).
1908-1923 Professor of Physiological Chemistry.
1923-1927 DeLamar Professor of Physiological Chemistry.

When Jones was made full professor in 1908, the Department of Physiological Chemistry was created. The new title, conferred on Professor Jones in 1923, commemorates the School's benefactor Captain DeLamar. The choice of the Professorship of Physiological Chemistry to receive his name, as one of several means of commemoration, gives recognition to Captain DeLamar's acquaintance with one branch of chemistry and to the specific part of his will that refers to his interest in nutrition.

The order of arrival of those who assisted Professor Abel and were officially designated as Assistants in Physiological Chemistry were Thomas B. Aldrich (1893-1899), Edwin S. Faust (1895-6), Walter Jones (1896-1908) and Arthur S. Loevenhart (1905-1908.[5]) During parts of this period Abel had but one assistant assigned to physiological chemistry. Indeed, during most of his term, Jones alone handled the teaching of chemistry. When Jones became a professor he had but one assistant most of the time up to the end of the World War. They were:

Arthur H. Koelker (1908-11)
Eli Kennerly Marshall, Jr. (1911-14)

[5] After 1908 Loevenhart stayed on for a time in the Department of Pharmacology.

D Wright Wilson (1914-1922)
Annabella E. Richards (substituting for Wilson 1918-19 during his leave on military service)

After the World War there came
Mary Van Rensselaer Buell (1921-1930)
Marie E. Perkins (1921—)
Lawrence Wesson (1922-25)
William Hoffman (1922-27)
Herbert O. Calvery (1925-27)

This roster does not include an occasional assistant not listed in Jones's own prospectus of the course.

The list is of interest itself, and also because its comparison with Jones's bibliography proves that, prior to the last period of his work, members of his staff seldom entered his field of research. Indeed he advised them not to do so.

After Doctor Jones had been with him a few years, Professor Abel advised him to study under Kossel. This advice was accepted but a financial difficulty stood in the way. Indeed there were times in Walter Jones's life when he had to be so absorbed in saving money that he feared he was becoming a "penny pincher". He related that when this fear was felt he had great satisfaction in throwing a penny into Jones Falls [6] as he crossed the bridge on his way to work. Appreciating the financial difficulty, Professor Abel had an inspiration one day when a sheriff appeared with the organs of a woman suspected of having been poisoned. While Abel was loath to have his laboratory burdened with such cases, he arranged to have Jones take this case. When Jones had identified strychnine he went to the distant town with his evidence. He found the people in an uproar over some scandal and a trial impracticable to hold, so he returned disappointed. At last he testified and brought home the needed fee.

The visit to Germany was short, June to December, 1899; but within that period Jones accumulated the data for two papers and became so inspired by Kossel that he devoted himself thereafter exclusively to the study of nucleic acids.

[6] The stream that runs through Baltimore.

It was Dr. P. A. Levene, the other American leader in this field, who welcomed him as Doctor Levene himself tells in the following letter.

"My memory pictures the arrival one day of a lean, tallish American of rather indefinite age, somewhat forlorn in a foreign land which he was visiting for the first time and, as I soon found out, having with him a wife and child. As my own sojourn in the city of Marburg had already had a history of a month or six weeks, I considered myself called upon to help Jones and family to find living quarters which was not a very difficult matter since the lady who sheltered an Englishman and myself, both of us working in Kossel's laboratory, was only too glad to make room for an American who could afford to bring a family with him all the way across the ocean and who, in her imagination, must be one of the American millionaires.

"The events that follow are rather vague in my memory and I am certain the fault is mine for Jones was not a man to permit a day to pass without leaving some impression on it. However, it so happens that besides Jones, two Englishmen, and one Italian, there were in the laboratory three Russians, a Frenchman, and myself and somehow, the Franco-Russian alliance is more vivid in my memory than that of the rest of the 'Internationale.'

"I remember clearly that as soon as Jones discovered that English was the dominating tongue of the Internationale—the Professor speaking English and the Russians being silent in every language but their own—he dropped his shyness and was ready for argument. Great enthusiasm and force of expression revealed themselves soon and, before long, I became a victim of them.

"Jones and myself shared a long laboratory bench but not too long to prevent occasional discussions. Jones was given the concrete problem of making derivatives of thymin. The work was progressing successfully and was destined to shape his principal interest in nucleic acids. My own problem was rather fantastic for I conceived the idea that vitellin must contain the chemical nucleus of nucleic acids since the nucleins developed during the growth of the embryo in the yolk. The problem being fantastic, and originating with myself, it naturally progressed poorly. Kossel was of much help to Jones and little to me. Jones became a devoted admirer of Kossel. I was more impressed by Hofmeister who, an enthusiast of the type of Jones, was certain that he held the key to the solution of the structure of the protein molecule. Jones was an arguer by nature; argument is the Russian sport par excellence. We had a lively argument and I was floored in the first though protracted bout. To save my ego, I blamed my defeat on the fact that Jones had the better command of English.

"So the friendship of Jones and myself began with an argument and continued in the same way for many years for we were warm friends regardless of our temporary scientific disagreements."

In the year of Jones's pilgrimage (1899) began the long series of papers on nucleic acid chemistry that constitute far the greater part of his contributions to science. These will be reviewed later.

Here we may dwell upon the sociology of his scientific research and teaching because it throws the light of one, somewhat isolated, case upon an important era of American physiological chemistry.

The views that Walter Jones held with respect to his dual rôle as an investigator and as a teacher, the adjustments that he made in relating a rapidly growing science to pedagogical problems, indeed, his whole attitude toward his position, present an enigma. Testimony conflicts and conflicting with that testimony which would make of Walter Jones a man interested only in nucleic acids stands one remarkable fact: the few letters that have come to light contain little of interest in regard to nucleic acids or similar scientific matters and much that bears upon his pedagogical problems. There is too little of this for one to dare a reconstruction of Walter Jones's views. If this sole body of documentary evidence is to be used, quotations must be placed against the background of his scientific career.

We may mention first the theoretical attitude toward chemistry in the new school. Perhaps there is no better way to do so than by reference to the addresses of Professor Welch,[7] for, while Welch may have appropriated many of the ideas that he expressed, no one preserved better their perspective in the general scheme and the very fact of some appropriation made him recognized as a spokesman.

Considering the time (1894) at which he spoke, Welch displayed amazing perspicacity in the following remark.

"Physiological chemistry means much more than what is usually taught in our medical schools as medical chemistry, which includes little more than the chemical analysis of certain fluids of the body for diagnostic purposes."

[7] William Henry Welch—Papers and Addresses, Vol. III.

Continuing, Welch put the seal of his approval on the following quotation from Hoppe-Seyler.

> "I cannot understand how in the present day a physician can recognize, follow in their course, and suitably treat diseases of the stomach and alimentary tract, of the blood, liver, kidneys, and urinary passages, and the different forms of poisoning, how he can suitably regulate the diet in these and in constitutional diseases, without knowledge of the methods of physiological chemistry and of its decisions on questions offering themselves for solution and without practical training in their application."

These quotations express in fairly definite terms the attitude that led the authorities to give a carefully considered place to chemistry in the medical curriculum.

There was also considered the important factor of the student's preliminary, liberal education of which Welch wrote that ". . . it should not be taken with reference to any utilitarian purpose. . . . when studying chemistry it should be done with the object of learning the general principles."

In general the effort was made to keep the Medical Department more than nominally a part of the University; ". . . ideals of the university must inspire the whole life and activities of the medical department." These ideals were to permeate the clinical departments where medicine should be treated as "one of the natural sciences".

All this had a great deal to do with the relation of Walter Jones to his students. It set up a guiding syllogism the first parts of which were made visible in the School's catalogue.

1. "The medical art should rest upon a suitable preliminary education and upon a thorough training in the underlying medical sciences."
2. Chemistry was made an important member of the "underlying medical sciences" by imposing an unusual entrance requirement, and by making physiological chemistry a prerequisite to the study of clinical subjects.

The third part of the syllogism was not stated in the catalogue but there was clearly the implication that a body of thought, the initiation of which had been so carefully planned, would be

carried on to the final objective. Welch stated this in 1899 when, after having previously indicated the advantages of keeping the pre-clinical subjects basic and free from too many clinical applications, he said:

"The real aim of medical education should be the training of practitioners of medicine and surgery, and the benefits of thorough grounding in the fundamental medical sciences are to a large extent sacrificed if the student does not find in the latter two years of his undergraduate study well-conducted clinical courses which afford opportunity for the practical application of knowledge previously acquired in the laboratories." [8]

Whether Walter Jones thought out the position of his department in this educational scheme or merely slid into the policy that he followed is immaterial when set beside the fact that he did seek to train students in what he considered the fundamentals and did leave clinical applications to be taught where used. In principle, this was in harmony with the general scheme. In fact he applied, however imperfectly, that part of the declared syllogism for which he was responsible. Could it be completed? The answer depended upon an appreciation of events.

Welch realized that physiological chemistry had replaced what he called "medical chemistry." If we continue to discriminate, as Welch did to emphasize developments, we shall find that during Jones's career there developed need of further distinction within chemistry. While the older physiological chemistry arising in medical centers had played with the fringes of clinical subjects, its more substantial new contributions were bringing the basic science to those developments of its biological phases that became the foundation of *several* branches. Among these was *clinical chemistry*. Furthermore, as the several parts of biochemistry's application carried topic after topic out of the hands of the medical profession and into the hands of special groups, the evolving clinical chemistry became, theoretically, the particular concern of the medical profession.

Within the period of Jones's career new analytical tools had made possible sufficient knowledge of body components to provide several new categories of thought associating material

[8] Welch: Papers and Addresses. Vol. III, p. 68.

changes with what is observed in disease. General principles of physical chemistry attained the power to deal with the interplay of multiple components as disease shifts equilibrium states throughout the body. There accumulated vast arrays of facts requiring sifting by clinical experience and, where possible, reduction to order by the logic of the basic science. Withal, a new perspective developed; sometimes the special science had to take courses of its own but perhaps as often it bore out the remark of Whitehead. "The paradox is now fully established that the utmost abstractions are the true weapons with which to control our thought of concrete fact." In all events, the broader knowledge had given scope to a type of thought known of old to clinicians but requiring discipline in new bodies of fact and logic. What Welch had decried as "medical chemistry" should have been relegated to special technical laboratories. Physiological chemistry remained essential, but, speaking broadly, it had expanded as a general subject, very unevenly and without any particular objective. Clinical chemistry was becoming a distinctive body of logic with attendant bodies of fact, not only centered on one objective but drawing its power from the broad concepts of the basic science and from techniques derived therefrom.

How much of this did Jones have in mind when, writing of the attitude of his department, he exclaimed, "Chemistry is looked upon here as the key to modern medicine."? It is reported that on occasion Jones would carry a topic from its academic beginnings to its decisive place in modern practice and that he was not above extravagant praise of the practical accomplishments of his science. This occasional jangling of the keys may have caught the attention of some students but such attractions were not needed by others. Jones made his science fascinating and it is testified that many a student was surprised to find the text so dull when the lecture had been so interesting. Here was stuff for educators to conjure with.

But the conjuring would remain lost motion so long as the new science retained only in theory the position envisaged by Welch and Abel and Jones. A student still could ask: Is a knowledge of chemistry *vital* to medical practice? It seemed a sufficient

answer that practitioners who declared their admiration of advances in chemistry forthwith would express regret at not having kept up.

Thus, Walter Jones was left to deal as best he could with that academic interest in chemistry which felt little pressure from the profession. He was led to write, "The younger men here usually take their chemistry as a perfunctory part of their medical education." As late as 1923 he complained, "Physiological Chemists are much more difficult to secure than either pharmacologists or pathologists for the reason that most students of physiological chemistry are headed toward medicine and do not stop in the former science long enough to become expert."

Had Jones taken more graduate students he could have counted on an established seriousness toward *general* biochemistry. But it would have been too much to have expected him to do so. He had too much to do almost single handed. Nor would this support of general biochemistry have met ultimately the basic cause of his complaint.

It is true that during many years the catalogue announced elementary and advanced courses in Physiological Chemistry for graduate students; but throughout the Medical School the intent of all "graduate" courses was to provide advanced training for physicians. It is not clear that any physician took Jones's offered graduate course. It is certain that only on very rare occasions was a graduate student accepted. The graduate students in West Baltimore felt that they were not wanted in the laboratory in East Baltimore and there are letters informing applicants that the department offered no training leading to advanced degrees.

Replying to Professor Henry Harper's inquiry of what he was doing for graduate students, Jones wrote in 1922: "Physiological chemistry . . . is treated exclusively as a medical subject and the entire resources of the Department are devoted to medicine. No attempt is made to train men for philosophical degrees." Thus Walter Jones was completely committed to the first parts of the syllogism framed for the training of medical students.

But also he had little inclination to be concerned with the concluding part. He had entered the school as an organic chemist with no particular interest in medical affairs, without specific

training in the evolving science of physiological chemistry, without recorded training even in biology, and he was valued initially and throughout his career as an investigator. Brutally honest with respect to his interests he nevertheless felt at home in the medical school not only because his teaching fitted the declared scheme but also because of one of the outstanding characteristics of university life at that period.

Americans had taken from an imported system only one of its better parts. This they had converted to an enthusiasm for research so furious as to have swept aside other parts of scholarship. The salutary effect of the transition from didactic teaching and from book learning to direct demonstration and observation in the laboratory needs no review. It was gloriously great and Jones gloried in being a part of it as an investigator in a new field On the other hand the trend was so impetuous as to have obscured the simple fact that, by no stretch of the imagination, can any student reconstruct for himself a small part of what the experiments of others, now and in past ages, can give him as his scholarly heritage. In its new freedom American scientific scholarship retained no certainty of how to make research subserve the whole of scholarship. As Welch warned in 1888, the encouragement of research is not the primary conception of the true university ideal. Possessed of a native genius for organization Americans elaborated their organization of research and gave little heed to the consolidation and reorganization of the knowledge acquired. Occasionally pedagogy went wild in the lower schools for lack of adequate organization by the higher schools of the *knowledge* deserving emphasis and logical placement in the progression of training. In the higher schools datum piled on datum and theory went the way of the convenience of specialists.

Jones was distinctly a specialist and was honored as such. He also took infinite care to consolidate in his monograph the knowledge of his field and he took great pride in having done so.

For lack of consolidation the attainments of research in the field of *clinical chemistry* went as isolated contributions, not as parts of a whole; as new specializations in an over-specialized culture, not as a group of evidences from which could be drawn

principles that permeate. The methods of chemistry would be conceded their uses in medicine, but clinical chemistry, then unrecognized as a distinctive body of thought, would have to await leaders strong enough to overcome professional inertia.

Those who knew Walter Jones, the general situation, and the unique position given to chemistry by the designers of the School will feel the power of exceptionally restrained expression in the following note with which he returned a widely discussed report on the teaching of clinical subjects in America.

"I supposed, at first glance, that I would receive some information from the report on the teaching of physiological chemistry, but it deals with the subject only in so far as it is submerged in physiology."

Also there was accumulating too much to teach and there remained too little time in which to teach even a part of it. American biochemists were becoming a little noisy in their demands for longer introductory courses. Professor Jones could not be drawn into this. Not only did he accept (perhaps passively) the plan for shortened courses in all departments agreed upon at Hopkins shortly before his retirement; he also wrote sometime before as follows:

"I should say that the time devoted in the medical curriculum to physiological chemistry depends to a considerable extent upon what portion of this subject is taken up in other departments on the borderline. I think the tendency has been to teach clinical medicine in departments of physiological chemistry and this greatly lengthens the time allotted to the subject." [9]

The curious position in which Walter Jones found himself should now be evident. The vigor of his research fitted the temper of the time. Its quality brought encouragement, promotion and fame. Valued primarily for this he was given a pedagogical problem of the first magnitude involving the cultivation of such an appreciation of a developing science as would subserve professional ends that were in view but that were only vaguely recognized within the profession. Doubtless he was given this task with full recognition of his genius as a teacher.

[9] Letter of February 16, 1921.

Yet to the historian of his time it could appear that students were then regarded as needing only exposure to original minds. The year was divided so as to give Jones ample opportunity for original research. But during the teaching period he handled large classes, at first single handed, then with one assistant. There is no evidence that he felt an incongruity between this and his complaint to Dr. Abraham Flexner of The General Education Board that medical students seem to have been ill prepared in college because they had "been crowded into large classes where they are expected to work under the poorest conditions. . . . They give excellent evidence of never having received the individual instruction which is so necessary in the . . . education of most men." It was set in the scheme of affairs that he should teach principles, not practices. There was provided no systematic way to build practice on the principles that he taught. He and many others were constructing the parts of a science that were to remain more or less unarticulated. The man himself had two major academic interests—nucleic acid chemistry and good lectures. He was somewhat indifferent to the rest. Still more indifferent to anything but research had been several of his preceptors and were several of his colleagues.

In summary it may be said that Walter Jones was not a student of medical education; he was an inspiring teacher. He was not a propagandist of an evolving science; he was a creator of one of its parts.

The conflicting opinions on the apportionment of his thought and effort to each phase may arise from the asking of questions that seem inappropriate when once the setting of his work is appreciated. Thus, it has been asked whether Walter Jones should not have created a school of physiological chemistry when the period of his career was favorable thereto. Since one yard stick used to measure a teacher is the stature of his pupils in his field this appears to be a fair question and a negative answer a detraction. But let us examine the facts.

If the reader cares to study the list of coauthors in Jones's bibliography he will find the following. The list includes the names of several members of the clinical departments. In the early years of the School the "preclinical" laboratories were cen-

ters of a good deal of research by members of the clinical staffs. Later, the introduction of the "full-time" system in clinical departments was accompanied by a more systematic organization of research in those departments and the participation of clinicians in the types of investigation that were influenced by the points of view in the basic sciences measurably declined While the going was good Jones cooperated.

The greater part of the list of coauthors is made up of medical students. The list of these medical students, some of whom Jones literally snatched from the bench of the introductory course to ensconce in his own laboratory, proves him to have been a good picker and sympathetic with the School's ideal of association between teacher and student in the rôle of coseekers. Of the fourteen medical students and four graduate students who published with Jones, nine attained academic distinction by professorial rank. One became a Nobel prizeman. It is not intended to say that Jones was mainly responsible for the training of these individuals. For example, among the medical students Whipple and Winternitz owe far more to Welch. It is intended to say only that Jones selected wisely and contributed much to the training of those who made this remarkable record.

On the other hand when we look at the careers of the *medical* students who published with Jones we find that none is distinctly in the field of clinical chemistry. To be sure, several of those who worked with Jones as members of his staff, as visitors, or as graduate students and several of the medical students who took his elementary course carried on with distinction in one or another field of chemistry or with the tools thereof. What is now under discussion is the influence of close association with medical students as judged *only* by the record of joint publication. An examination of the circumstances already cited will not detract seriously from the esteem of Walter Jones as a teacher; it will reveal the weakness of an evolving system.

Under the circumstances the trend of Jones's intimate students is not difficult to see. They were well initiated by the introductory course in which Jones hewed to the line of the established syllogism. Then Jones gave them an invaluable introduction to the serious investigative method. Finally, they came to the

clinical departments. There clinical chemistry was merely a valued adjunct, not given a position commensurate with that of morphological sciences. There established sciences had their scholars. Within their fields both the universal experimental method and the knowledge culled from the trials of ages were united in one objective. Few students could have resisted this. None deviated from the organized path to seek training by paths not organized. Such, perhaps, is one perverse manifestation of the American genius for organization.

The testimony is almost universal that Walter Jones was a brilliant lecturer. It is not equally well appreciated that he took great pains to prepare the materials of his lectures and that he did not spare himself in developing their eloquent logic. In the closing paragraph of a letter to be quoted later Doctor Read writes of those moments before a lecture in which a great emotional strain was evident. Before that, however, there had been a long period at his favorite place before the balance where he worked out the flow sheet of what was to appear as a smoothly developed subject. Perhaps they were his eloquence, his gossipy reviews of controversies, his sharp wit that brought the advanced students back to the first-year class room; but it may be doubted that these alone induced them to come year after year. There was an intellectual interest. Robert P. Kennedy writes:

> "Walter Jones's lectures, both in class room and in private conversation, were virile and impressive. No matter what sort of tirade his dissertation may have sounded like, his thoughts were logical, his expressions extremely accurate and if the listener were inclined to argue he was always worsted by a better piece of argumentative effort."

Professor Wright Wilson, while an assistant to Jones, noted the frequency of Jones's new approaches to old subjects and the dramatic quality of the lectures which were "made so interesting that students of upper classes often returned to hear him speak."

The vividness of certain impressions created during those lectures is illustrated, perhaps extravagantly, by the effect of his comparison of combustion in a mouse and a candle. A mouse and a candle were brought near to extinction under bell jars

and then revived in air—all in pantomime. A student today swears that the experiment was real! "And the wonderful thing is this", said Jones, "one has to light the candle but one doesn't have to light the mouse."

There were times in the early years when descriptive material loomed rather large in the course. Physiological chemistry was then largely descriptive. There were times in the later years when nucleic acids loomed rather large in the perspective. Emphasis here had the advantage of giving the students an insight into a subject in the making as presented by a maker and what they lost in general perspective was compensated, as many of them have attested. When others complain of such restrictions let them not forget the times.

In view of Walter Jones's training, his preoccupation with teaching and with highly specialized research, it is not surprising that his technical knowledge of the evolving applications of physical chemistry was limited. He was blatantly honest in all such matters. But I can testify that in the later years he had an appreciation of its developing importance. The following letter to Doctor Holt reveals both sympathy with one development and a characteristic regard for his responsibility to students headed toward the clinical uses of chemical thought.

"December 12, 1923.

"Dear Doctor Holt:

"After thinking over the matter that has been a subject of conversation between us and talking it over with a number of people I have come to the conclusion that the physical chemistry of proteins, as it now stands, is rather too special a matter for our second year students. Of course, an optional group of men can be gotten together for any course, as students will take your word if you say that the subject treated is of importance in medicine.

"I see no reason, however, why you should not give this optional course on your own accord and in connection with the Department of Pediatrics. I am in sympathy with it and would encourage it in every way except as concerns matters above stated.

"Very sincerely yours,

"Walter Jones."

The usual testimony is that Professor Jones was a stimulating teacher and not a soft one. One student will not forget the moment he poured the materials of an experiment down the sink, explaining, "It didn't work the way the book said." The Professor exploded, "My God, man, if you threw a brick out of the window and it went up instead of the way the books say, wouldn't you stick your head out?"

Occasionally, Jones was accused of being a bit too severe with stupid students. Another aspect is revealed in a letter regarding a candidate for an advanced degree who was subjected to an oral examination by Professor Jones. After stating that he was not satisfied with the candidate's knowledge, Jones added:

"The matter is a little annoying to me and I would not make a report of this kind if there was the slightest possibility of making any other. I suggest in Mr. ———'s interest that you substitute me on the Committee by someone else and see if a disagreement among authorities cannot be produced."

Jones could be mischievously enigmatic. To Doctor Mendel, Editor of the *Journal of Biological Chemistry*, he wrote, when forwarding a paper:

"If you will read the last page of the long article you will concede the desirability of concealing not only its contents but even the fact that I am publishing any article at all."

When admitting a clearly explained mistake of his own that Wilson had noted, Jones bubbled over with an old jibe.

"Dear Doctor Wilson:

"If you have as much difficulty in calculating in dollars and cents as you have in reckoning normal solutions, I should think you would get along pretty well in dollars and cents. Your calculation is exactly right. I now see the reason. I made arithmetical mistakes all the way through. So everything along the line is now beautiful."

It is difficult to convey the nuances of Walter Jones's acts and utterances. He would press to the hilt the thrust of his argument. If the victim felt wounded he had still to discover that Jones's glee in the game made him unconscious of a personal aspect and in time he would discover that Walter Jones could

melt into the very essence of thoughtful kindness. Again it might be thought a typical Americanism when he bullied a student into a position where he could confer on him a great favor, or it might be thought sweetness of a soft kind when he dried the tears of a secretary whom someone had offended unjustly. Not so; with many an act or word of kindness would go some unique remark that devastated the occasion for the kindness shown. Many a witticism drags in the telling for the simple reason that few can reproduce the relations of circumstance and effect on the listener. Frequently, it was only when blood oozed that the listener realized there had been a rapier thrust.

The following dialogue is reported by Doctor Rowntree to have taken place when Osler returned to Baltimore and Rowntree led him to Jones's laboratory.

Osler Well, Doctor Jones, so you are still wasting your time playing with test tubes. One of these days the Grim Reaper will come and afterward people will say: "Didn't Walter Jones do this?" Later they will say: "Didn't Jones do this?" Finally they won't know who did this.

Jones I see you've become a pessimist since you left Hopkins. No, Doctor Osler, we are not forgotten. Only the other day someone asked me: "Didn't Hippocrates do so and so?"

Osler Hippocrates! He was one in a hundred million.

Jones Oh, well, there are others. Every week some one says, "Galen did that!"

Osler Ah, but Galen was one in a hundred million!

Jones By the way, Doctor Osler, how long is it since you left Baltimore?

Osler Five years.

Jones I'll swear I heard your name mentioned in those five years.

Walter Jones's early training developed an intense idealism; the later training turned its values to the service of a "tough-minded" realism. The incompatibles encountered on the way gave his conversational abilities their chief opportunity, but they seem to have had a very deep significance for the man himself. Unfortunately, we can judge this only from the impression of a profound intellectual struggle. It is hard for me to believe that

the constant recurrence of all sorts of profound questions in his conversations meant only the creation of an opportunity to talk, as some would have us believe. Nor can I see much in individual citations of the way topics were handled. In their isolation they often lack point. It is the impression of a constant hammering that seems to denote perpetual struggle. What would appear on the surface and without warning would be something of the following order. A colleague, bumped into while passing a doorway, is confronted.

Jones, opening and closing a jack-knife, "Tell me, What do you see me doing?" A facetious reply is stopped half-said. "Answer me! What do you see me doing?"

A. "Why, Doctor Jones, you opened the knife; then you closed it."

J. "Of course, and now tell me this. When I opened the knife, what became of the closedness? When I closed the knife, what became of the openness?"

A. "But, Doctor Jones, that's . . ."

J. "Of course it is; but that's Kantian philosophy!"

No one would claim that Walter Jones was a profound thinker. Everyone will admit that he hit with deadly aim the nonsense current in the common ideas of his time. They might be ideas of primordial creation or the square root of minus one that were the topics of discussion around the lunch table. It was not his object to expound but to take the ideas that were current and lash the nonsense unmercifully. To find the significance of the fact that the square root of minus one irked Walter Jones one must not think of him dealing with that quantity's logical arrival, its place in a broad theory of numbers, or its practical use, as in electrical theory. One must recall the verbal logic without attachment of extended meaning fed to most of us in the texts of our youth—that verbal nonsense which still permeates much elementary instruction today and bespeaks the failure of the universities to organize advanced learning in such a way as to make its reduced elements senseful. Of like significance is the story of Jones's argument with a physicist over the meaning of "dextro" and "laevo" with reference to the rotation of the plane of plane-polarized light. Since few advanced treatises record the historical change of convention and hardly any text states

any convention clearly, it may have been natural that one of the disputants followed the older and one the newer convention. Walter Jones clinched his argument by rushing to his polariscope with a solution of D(d) glucose [d(+) glucose] and pointing to the direction in which he rotated the analyzer.

So, in all matters, Walter Jones demanded attachment of significance to words,—where possible, significance that could be made concrete and demonstrable. There at last he found his joyful satisfaction and his willingness to forego the delusive satisfactions of the philosophic WHY. Said he, "No question in biology that begins with *why* will ever be answered."

Yet he never questioned the satisfactions of others. Because his friends in all callings felt this, they would allow Walter Jones to hit his hardest. And hit he did to the consternation and discomfort of the unknowing.

In the fall of 1923, Doctor Jones wrote of "a remarkable weariness in my left leg after walking a few blocks." It soon became evident that he was suffering from thrombo-arteritis and in January he was unable to meet his classes after the first lecture. Thereafter he appeared at the laboratory only to discuss general matters. The unaccustomed restraint worried him no end and finally he admitted, "I have a case of 'nerves'". Then he abandoned work entirely. He retired in 1927 and recovered his health slowly and incompletely.

In 1921 tentative plans had been made for "a new building for physiological chemistry". The immediate necessity arose from the expanding work of the Departments of Physiology, Pharmacology and Physiological Chemistry and of the offices of administration, all housed in the Physiology Building. Chemistry was confined to the attic. The state of affairs there may be imagined from the fact that the hood available for students was so badly ventilated that adjustments of Kjeldahl digestions had to be made between rushing into and out of the room with bated breath. The purveyor of student supplies could hardly turn around in his coop. Every inch of space, including that under the eaves, was used, and two polariscopes were set up in a room so small that the doorway was in constant use as a means of turning around.

Therefore, it should have been with high hopes that Professor Jones accepted the chairmanship of a committee on plans for a new building. Nothing concrete was accomplished until after Jones's retirement. Then, after the completion of the "New Physiology Building" in 1929 there occurred a little incident that should be recorded.

After a long absence from his old department, Walter Jones appeared to lunch with the new staff. As of old he held the course of conversation, and long enough for a secret messenger to obtain flowers for the laboratory that had been reserved for the Professor Emeritus. Then he was led to his new quarters. There Walter Jones became for a moment his old self sending the faithful "Andy" on trips for glassware and reagents. Suddenly, he disappeared never to return. Later it was learned that there had come over him a discouraging sense of ground lost and of lack of energy where energy had known no bounds. Nor could he be induced to try again where once the sense of commanding superiority had been lost. He knew then that he had retired completely to pleasant places on the shore or in the mountains during summer months and that music and bridge must keep him diverted in the winter. For excitement, he played with the stock market or drove his car "two thousand miles a month" searching all the byways of Maryland's beautiful countryside.

Walter Jones died in Baltimore, February 28, 1935. Mrs. Jones died the following year, November 18, 1936.

Before we take up the record of his scientific work, let us revert to the happier days before the incapacitating illness as those days are described in the following letter from Dr. B. E. Read.

"Henry Lester Institute of Medical Research
"Shanghai, China
"May 25, '35

"Dear Professor Clark:

"I regret that foreign travel has kept me from storing old letters and that I have not preserved any of Walter's delightful compositions.

"It was upon Doctor Welch's recommendation that Professor Jones was willing to accept my unworthy self to work in his

laboratory in 1916 and 1917 for eighteen months' intensive study of nucleic acids and derivatives. I was privileged to enjoy the warmth of his friendship which for years afterwards he sustained by an occasional postcard or message through mutual friends.

"Jones's appreciation published in Science [10] is an admirable summary of his rare qualities. I, for one, realized quite frequently that his warm personality could be scorching hot. In his contacts with his fellow men his mind acted as a refiner's furnace, from which the more timid shrank, by which the unaware were rudely surprised, and in which the more courageous found a degree of salvation.

"He had an intense love of his fellow men unappreciated by the victims of his assaults,—assaults upon everything small, weak or mean in men around him. At the conclusion of his speech at the New York dinner of the Federation in 1917 he confessed his own creed in the words:

"'This above all: to thine own self be true,
And it must follow, as the night the day,
Thou canst not then be false to any man!'

"Walter Jones's contemporaries will witness to the high order of his intellect, thousands know of the brilliance of his classroom talks, a few, like myself, were privileged in a remarkable way to know his daily round of sound reason and extravagant wit. Whilst Walter Jones isolated himself from his fellows more than any scientific man I have known, intellectually he saw them and himself as one in the great struggle for the triumph of reason; and underneath his mocking laughter was a human sympathy of a very fine order.

"I had more courage than usual one day when I said to him, 'Professor Jones, I saw an unusual example of American spelling this morning.'

"'Oh, what was it?'

"'A negro fish shop on Monument Street had a notice of "CLAMPS" for sale.'

"Jones's reply was instantaneous, 'Oh, that's nothing. When I was in England they gave me lead in my plum pudding.'

"That kind of banter made guanylic acid not quite so sticky and the morning's work passed quickly and Wright Wilson, Sam Goldschmidt and Eddie Plass would gather round the ham sandwiches for a session of seasoned wit more carefully considered and narrated.

[10] Science, March 29, 1935.

"Before his brilliant lectures Jones would pace his private laboratory in agony like a mother praying for the life of her child. More intimate details would reveal the fact that in his synthesis of thought and feeling Walter Jones exhibited real genius.

"Sincerely yours,
"Bernard E. Read."

AFFILIATIONS WITH LEARNED SOCIETIES

The National Academy of Sciences. Elected member in 1918.
The American Physiological Society. Elected member in 1898.
The American Society of Biological Chemists.
Councilor in first group of officers, 1907, and in 1920 and 1921.
Treasurer 1910-12.
President 1915 and 1916.
Journal of Biological Chemistry: Editorial Committee, 1905 (vol. 1) to 1929 (vol. 84).
Society for Experimental Biology and Medicine.
Elected member 1905. Resigned 1921.
American Association for the Advancement of Science.
Elected member 1928, Fellow 1931.
Phi Beta Kappa. Elected member 1906.
Sigma Xi. Signed the petition for the establishment of the Johns Hopkins Chapter. No record of membership.

SCIENTIFIC WORK

Walter Jones's dissertation (1891)[11] describes the preparation and analyses of some new sulphonphthaleins. The work was part of a series of investigations being carried out in Remsen's laboratory and is of little importance beyond that extension of the record which is necessary in such cases.

As an associate of Winthrop E. Stone at Purdue, Jones was co-author of a paper (1893) regarding the digestibility of pentosans. In 1894 he published from Purdue a note regarding the dichlorides of orthosulphobenzoic acid. Others had reported that the reaction of phthalyl chloride with "hydrosulphide" yielded

[11] Published in the *American Chemical Journal* (1895).

$C_6H_4 \begin{smallmatrix} CS \\ <\!\!>O \\ C{=}O \end{smallmatrix}$. Jones expected a similar compound on similar treatment of a dichloride of orthosulphobenzoic acid but he obtained $C_6H_4 \begin{smallmatrix} CH_2 \\ <\!\!>O \\ SO_2 \end{smallmatrix}$ under various conditions. Apparently Jones had hoped that he might obtain additional evidence of the isomers of the chloride of orthosulphobenzoic acid which had been under active investigation in Remsen's laboratory but no definite contribution to this subject appeared.

Jones's first paper from "the Laboratory of Physiological Chemistry of the Johns Hopkins University" was published with Thomas B. Aldrich as senior author and describes the isolation and identification of α-methyl-quinoline as a constituent in the secretion of the anal glands of *M. mephitica.* This was an addition to the list of nitrogenous secretory products.

Many years later W. Hoffman continued work in Jones's laboratory on the secretion of the anal glands of the skunk. Then people in neighboring laboratories recalled the earlier days and were made aware of the inadequacy of the hoods in the chemical laboratory. One bottle of skunk secretion, shipped from West Suffield, Connecticut, was broken in transit and was followed by a letter from the shipper saying:

"I sent one pint and box was proper but I have had H— from Express Co."

Jones next turned to a study of melanins, following an exploration previously made by Abel and Davis. Jones stated in the opening sentence of his first paper: "The name 'Melanin' is a generic term which is used to include all the dark brown or black animal pigments, whether formed in the body by normal processes, or under pathological conditions." The chemistry of these pigments is by no means clear today. Jones's work upon them was largely preparative. By successively destroying other constituents of black horse hair, he prepared material that, when further subjected to a caustic alkali melt, yielded so-called melaninic acid. This was subjected to elementary analysis and

various characterizing tests. A second paper with Auer described the results of its oxidative treatment.

Perhaps it is not inappropriate to speculate upon what might have happened had Jones not found his talents in the next subject to which he turned. Find himself he did in the study of nucleic acids. That he might have continued in one field is not an improbable premise in view of the fact that after his enthusiasm for the study of nucleic acids had been aroused he never published on any subject not directly or indirectly concerned with these substances. That he might have continued his interest in the melanins is not improbable for they furnished an abundant opportunity for exploratory work to which Jones seems to have been adapted. Had he continued the way he had been going, he might have retrod the weary path of preliminary exploration that has proved necessary in the study of nearly every important group of natural substances and that usually comes to a blind alley until the state of the theoretical science attains new power to break through. It is fortunate that he was attracted to the nucleic acids. Enough was known of certain of their constituents to provide a sound basis for systematic chemical work. So much was not known that he was provided an abundance of opportunities. Existing discrepancies and confusions called forth Jones's peculiar talent.

Before considering the specific contributions of Walter Jones to our knowledge of nucleic acids it may be well to outline the state of the subject when he entered the field. Brief as this outline must be, it may aid in recalling the nature of the problems that he attacked and in revealing some of the objectives of his investigations.

The study of nucleic acids begins with the pioneer and classic work of Friederich Miescher. As a student under the anatomist His, Miescher was inspired to look beyond the range of the microscope for the chemical dimensions of morphological peculiarities and he went to Hoppe-Seyler's laboratory with his vision. He saw an opportunity to get at nuclear material by digesting the more easily attacked parts of pus and by this and other methods he obtained powders rich in nuclear material and having chemical properties unlike anything known before. What

Miescher considered to be the characteristic constituent he called "nuclein". In 1869 Miescher submitted a paper on this material to Hoppe-Seyler, the founder and editor of the *Zeitschrift fur physiologische Chemie*. Hoppe-Seyler held this paper for confirmation by his students and they soon separated "nuclein" from various sources. In the meantime Miescher was called to Basel and there recognized an opportunity presented by the Rhine salmon. During their ascent of the river the salmon develop their reproductive organs enormously and the males become a potentially huge source of nuclear material since the spermatic fluid, when expressed through the *vas deferens,* consists largely of a suspension of spermatozoa the bulk of which is of nuclear origin. Seizing the opportunity, Miescher prepared from this source samples of "nuclein" of definite and reproducible properties. He was led to believe himself in possession of a salt of a new organic acid and an organic base that he called "protamine". The base was later to be characterized as one of the simplest of the proteins while the organic acid was to become known as nucleic acid.

In 1899, when Jones began his work, a nucleic acid had been separated from the "nuclein" (or nucleoprotein) but "nuclein" rather than its resolved components was still the chief material used in various sorts of investigation.

We must leave much of the detail of Miescher's work to the reviews given by Jones and by Levene and Bass [13] and note that, among the numerous investigators who followed Miescher's pioneer work, the one who was to become Jones's mentor was Albrecht Kossel. Kossel provided Jones a fairly direct intellectual heritage of the field since he had worked under Hoppe-Seyler who in turn had received his start in the field from his painstaking and brilliant student Miescher.

It was Kossel who, in the period 1879 to 1886, first definitely identified purines among the products of the hydrolysis of "nuclein" and recognized their source in the part later to be called nucleic acid rather than in the protamine moiety. It was Kossel who first recognized a pyrimidine in the hydrolysate. With Altmann's preparation in 1889 of the non-protein component,

[13] Jones, *Nucleic Acids* (1920). Levene and Bass, *Nucleic Acids* (1931).

which he called nucleic acid, the material for a more rigid system of constitutional studies was provided. The protamines were to furnish Kossel and others one distinctive group of materials and the nucleic acids another. Jones was to work on the nucleic acids. Although he may have done work on the protamines, he never published it.

With the identification of purines and pyrimidines in the hydrolysates of nuclein and nucleic acid the story of the nucleic acids becomes linked with the story of uric acid which had been more than a century in the making. It is often said that uric acid had the attention of more leading chemists than any other compound. It was discovered in 1776 by Scheele, perhaps the greatest of discoverers in the field of chemistry. Its elementary analysis was made by Liebig, the founder of quantitative organic analysis, and a comprehensive study of its decomposition products was published by Chemistry's Damon and Pythias, Wöhler and Liebig, in 1838. No less a one than von Baeyer undertook and nearly completed one synthesis of uric acid and Emil Fischer completed not only this synthesis but also brought a comprehensive order to the structural relations of the purines as he did for other groups of compounds that are of the greatest bio-chemical importance. While these names claim the attention of every casual student of chemical history, the historian will find many another notable name associated with uric acid, with its close relatives in the purine group and with the next of kin, the pyrimidines. The labors of the great and lesser had provided a very substantial body of information directly applicable to the study of nucleic acids. Nevertheless, this information had to be adapted to the new situation, and while the definitive constitutional studies were still in the making.

In later years it was to be proved that the two better known nucleic acids contain residues of the following substances.

Thymus Nucleic Acid		*Yeast Nucleic Acid*
Phosphoric Acid		Phosphoric Acid
Adenine	(Purines)	Adenine
Guanine		Guanine
Cytosine	(Pyrimidines)	Cytosine
Thymine		Uracil
d-2-Desoxyribose	(Carbohydrates)	d-Ribose

Between the beginning of Jones's study (1899) and the identification of d-ribose by Levene and Jacobs ten years passed. Desoxyribose was found after Jones's work was ended. (Levene and London, 1929). Jones himself was never to be concerned extensively with the carbohydrate moieties. Pyrimidines were known as a group and had been dealt with extensively in the study of uric acid; but the first pyrimidine to be isolated from nuclein was thymine, which Kossel and Neumann had found in 1893. The study of its constitution was under way in 1899. Indeed, it was a study contributory to this that Jones was drawn into when he went to Kossel's laboratory as will appear later. Purines, of course, were known as a group and those related to nucleic acids had been found in various natural products; but while the important adenine had been discovered by Kossel in 1885, it was not till 1897 that its relation to uric acid was definitely established by Fischer.

In 1899 it was not proved that the known constituents of a nucleic acid are linked in one molecule. An individual nucleic acid might contain but one purine. There was little vision of how components might be linked together in one nucleic acid. Nucleotides, with the order of linkage phosphate-sugar-purine (or pyrimidine) were unrecognized as pertinent to the studies of nucleic acids. Two nucleotides, inosinic acid (Liebig, 1847) and guanylic acid (Hammarsten, 1894) had been isolated from tissue, but their structures were unknown. Indeed Jones was to mention guanylic acid for several years as a nucleic acid. Nucleosides from sources other than nucleic acids had been known since 1885 but their uses in unravelling the structure of nucleic acids was not fully realized until the work of Levene and Jacobs in 1909 when they were recognized as representing the arrangement sugar-purine (or pyrimidine) in nucleic acid.

Thus a good deal of spade work remained to be done and the absence of those clear-cut constitutional definitions which determine the course of many sorts of investigation left the subject alive with puzzling problems. Perhaps it was because he had been plunged into the midst of these that Jones opened his monograph on the nucleic acids with the remark:

"The early development of nearly every scientific subject is marked by a set of conditions under which it is extremely difficult or even impossible to distinguish the important from the unessential; and unfortunately any misapprehensions which in consequence arise are likely to be so engrafted upon the nomenclature as to perpetuate themselves automatically."

The subject was outlined very dimly during the early days of Jones's work. He found a literature laden with discrepancies. He dealt with material that presented difficulties still felt. There is the unique constitutional complexity. Also it is difficult to prepare nucleic acids in purity sufficient for some of the refined uses of analysis and when this desired end is approached the materials do not lend themselves well to ordinary physicochemical tests for homogeneity, molecular weight, etc. Recollection of all this is essential to an appreciation of Jones's work.

Before a review of Jones's specific contributions, the polemic cloud that hangs over much of his writing must be explained and dispelled. In 1908 Jones said, ". . . the literature on the subject reveals a mass of contradictions, corrections and inconsistencies which it would seem almost impossible to reduce to any satisfactory scientific order." Jones was determined to bring order; he could not endure disorder. Consequently, we find that many of his papers have their immediate objectives in the resolution of discrepancies now forgotten. Were too much emphasis placed upon them, a review could miss the major objectives.

Levene and Bass say of the field in general: "It is singular that in the history of the chemistry of nucleic acids each new conclusion was reached by a path of disagreements, controversies, and errors, and that error often led to progress."

There is a tradition that Jones once ordered the course of a lecture by writing on the black board the names of contributors to the literature of nucleic acids; then crossed out the name of each after discussing his mistakes.

Be that as it may, it was inevitable that the shillalah of this forthright man would occasionally hit a hard head and elicit a reply. To dwell too long upon the resulting controversies, or to pick up each argument where Jones begins on smaller issues, would be to confuse the main issues. The only loss will be to

make less vivid his part as a clarifier, a part too soon forgotten when order in a subject is attained and students of the subject have only to master established relations.

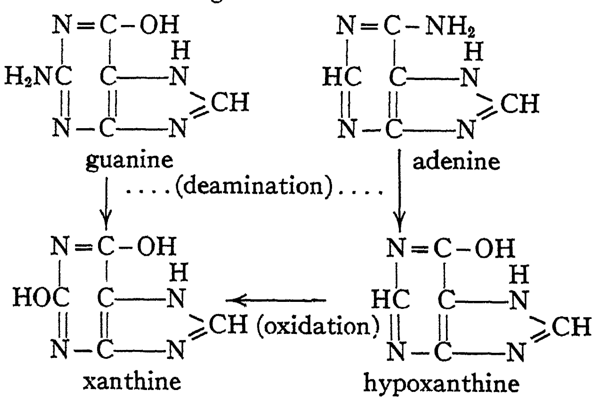
INITIAL RESEARCH ON NUCLEIC ACIDS

As already mentioned, Kossel had isolated thymine from a nuclein hydrolysate but its constitution was not known at the time Jones went to Kossel's laboratory. Jones (1899) prepared a valuable bromine derivative of thymine and confirmed Kossel's opinion that thymine is distinct from the 4-methyl uracil of Behrend.

Then Kossel suggested to Jones the preparation of thymine directly from tissue rather than through the intermediate isolation of the nucleic acid or nuclein. Kossel may have felt what Levene expressed forcefully in a lecture of later years; the advantage of accumulating abundant material before a study of constitution is begun. This was before the development of modern micro methods. Jones (1900) helped to accumulate thymine by successfully preparing it from hydrolysates of herring testicles.

ENZYME STUDIES

In studying the hydrolysates of tissues, of nucleins and even of nucleic acids, various investigators had detected the presence of oxypurines as well as amino purines. For present purposes we may confine attention to the four purines whose relations are exhibited in the following formulas.



Hypoxanthine is formed by deamination of adenine. Xanthine is formed by deamination of guanine or by oxidation of hypoxanthine.

Reports of the occurrence of these four purines and in proportions that maintained no uniform relation had led Kossel to the supposition that there are four nucleic acids each containing the residue of one purine. On the other hand, Schmiedeberg had been led by his study of salmon nucleic acid to the assumption that adenine and guanine occur together in one nucleic acid.

One, who, like the writer, is not intimately acquainted with details of the technical procedures, will have difficulty in judging the extent to which the presence of mononucleotides in tissues confused the issue. Indeed, Jones was later to deal with one aspect of this. But, whatever may have been the extent of this source of confusion, there remained a distinct problem in the relation of these four purines to true nucleic acids. Jones drew the first of the students who were to publish with him on nucleic acids into an examination of this problem. The student was George H. Whipple.

It happened that Abel was then studying the pharmacologically active principle in the medulla of the suprarenal gland. The unused residues furnished Jones and Whipple a source of a nucleic acid that had been reported to contain a methyl purine and to be free of guanine,—unlike any of the nucleic acids from other sources. From their own examination of this material Jones and Whipple (1902) drew the conclusion that the nucleins of the suprarenal glands of sheep and beef are similar to that of the pancreas and that each yields adenine and guanine but no demonstrable amounts of other purines.

Thereafter Jones suspected that the reported xanthine and hypoxanthine might arise from deamination of the amino purines. First, it was advisable to check previous observations. Jones (1904) found that while guanine, adenine and thymine can be identified among the products of hydrolysis of thymus nucleic acid, if acid hydrolysis is used, autolysis of the gland results in the formation of xanthine and hypoxanthine. This was checked by studies of the spleen and suprarenal. Then tissues were

examined for deaminizing enzymes. Jones and Partridge (1904) reported guanase; Jones and Winternitz (1905), adenase.

The justification for inferring specific enzymes for the free purines was chiefly of two sorts; first, the conversion of added guanine to xanthine and the conversion of added adenine to hypoxanthine by infusions of certain tissues; second, failure of the one process and demonstration of the other by specific tissue infusions. Thus, a spleen infusion brought about an alteration of adenine to hypoxanthine, presumably by the action of an adenase, and thence to xanthine in the presence of air, presumably by the action of xanthine oxidase. On the other hand, guanine remained unaltered in this infusion. In contrast to this set of results an extract of pancreas completely changed added guanine to xanthine.

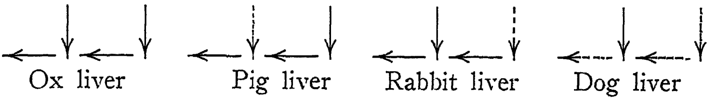
The failure of Jones to emphasize the species from which he obtained the spleen for his experiments led to some amusing and important consequences. Schittenhelm could not confirm Jones's report, but Schittenhelm had used ox spleen while Jones had used pig's spleen. When Jones discovered this he entered a controversy that determined the course of several of his subsequent investigations, for the sharp emphasis given to species and organ specificities led Jones and his students to extensive exploration.

The reader of Jones's papers will note that the initial controversy with Schittenhelm, beginning with the paper by Jones in 1905, is pursued in other directions. Indeed, the full flavor of it developed outside the printed page. Doubtless some of the cryptic remarks that appear in Jones's later papers and in his monograph lack clarity for the reason that their full meaning needed the expansive atmosphere of the lecture room.

The first findings in regard to the distribution of deaminizing enzymes are summarized by Jones in the following diagrams. Let the scheme of enzymatic activities be represented as follows:

$$\begin{array}{ccccc} & & \text{Guanine} & & \text{Adenine} \\ & & \downarrow{\scriptstyle(\text{Guanase})} & & \downarrow{\scriptstyle(\text{Adenase})} \\ \text{Uric Acid} & \xleftarrow[\text{(Xanthine oxidase)}]{} & \text{Xanthine} & \xleftarrow[\text{(Xanthine oxidase)}]{} & \text{Hypoxanthine} \end{array}$$

If a full line arrow represent the indicated enzymatic activity and a dotted arrow represent its absence, distribution may be shown as follows:



See Jones and Winternitz (1905), Jones (1905), Jones and Austrian (1906), Jones and Austrian (1907).

That the enzymes are formed successively during embryonic growth was shown by Jones and Austrian (1907), Jones and de Angula (1908) and by others, giving additional presumptive evidence of their individualities. This use of embryos has been followed in other biochemical studies.

Levene and Jacobs (1909-10) then demonstrated the order of linkage in inosinic acid to be: phosphoric acid—pentose—hypoxanthine. By acid hydrolysis they obtained a nucleoside. They discovered its pentose to be the hitherto unknown d-ribose (Levene and Jacobs, 1909-12). Then they extended their methods to yeast nucleic acids and obtained the four nucleosides: guanosine, adenosine, cytidine and uridine. There was now presumptive evidence that yeast nucleic acid is composed of the residues of four nucleotides in which the orders of linkage are:

guanine-ribose-phosphoric acid adenine-ribose-phosphoric acid	} purine nucleotides
cytosine-ribose-phosphoric acid uracil-ribose-phosphoric acid	} pyrimidine nucleotides

There was at hand a constitutional basis upon which to erect a working hypothesis of the enzymatic degradation of yeast nucleic acid.

In the period that follows, Jones recognized that deamination of the purine may occur either while the purine is combined or after it is set free. Having previously reported that guanylic acid is unaffected by pig's pancreas, as judged by the failure of that tissue to catalyze the liberation and deamination of guanine, Jones reinvestigated the matter in 1911 and reported the liberation of phosphoric acid. Therefore, he corrects the interpretation of the previous findings. Guanosine (not guanylic acid)

is unaffected. On the other hand, adenosine is converted to inosine by pig's pancreas, that is, deamination now occurs while the purine is combined.

In the same year Amberg and Jones (1911) showed that although dog's liver forms hypoxanthine from yeast nucleic acid (which would admit of either of two routes) it does not form hypoxanthine from free adenine. Therefore, it was assumed that the liver contains the deaminizing agent, adenosinase, but not the other deaminizing agent, adenase. It also contains the nucleosidase which, if specific, is inosine "hydrolase", but not adenosine "hydrolase".

On the other hand, Amberg and Jones (1913) found that yeast is unable to deaminize adenine whether free or combined. Yeast also shows the specificity of its polynucleotidase by attacking yeast nucleic acid but not thymus nucleic acid.

In Jones's lectures he often emphasized that an enzymatic action upon a specific group may depend upon the mode of linkage of the group carrier with some other residue and he anticipated in some measure what was to be shown subsequently regarding phosporylated metabolites in general.

The recasting of the scheme of enzymatic nucleic acid degradation, initiated by the constitutional investigations of Levene and Jacobs, was also dealt with specifically by Levene and Medigreceanu in 1911, and has since been further elaborated in directions that we need not follow.

The formulation of yeast nucleic acid as a tetranucleotide opened the possibility that a highly selective catalysis of its hydrolysis might result in the formation of dinucleotides or even a trinucleotide as well as mononucleotides. Jones and Richards (1914) believed they had found a dinucleotide resulting from the action of fresh pigs' pancreas on yeast nucleic acid. These "dinucleotides" were described further by Jones and Richards in the following year. The subject was to be followed further in those studies with unorganized hydrolytic agents that will be mentioned in another connection.

In the late period of his work Jones devoted less attention to enzymes. However, in 1920 he described a most remarkable experiment that indicated a thermostable agent in pigs' pancreas by the use of which yeast nucleic acid was split into its four

nucleotides and possibly into dinucleotides initially. (See also Jones, 1922.) Jones and Perkins (1923) returned to this as a means of studying constitution. Levene and Bass (1931, p. 312) state that in unpublished work Levene "was not successful in his attempts to repeat the experiments of Jones". There the remarkable observations on the thermostable agent seems to rest, but the repeated use of the procedure by Jones leaves no doubt that it deserves further study. After the comment on the thermostable agent was written, Jones's observation was confirmed by Dubos and Thompson. See J. Biol. Chem., *124*, 501 (1938).

The series of enzymatic accelerations leading from nucleic acids to uric acid (in man) has an obvious bearing upon the problem of gout and, while this problem involves other phenomena, the clarification of the enzymatic processes in man is a necessary part of the resolution. The few articles in which Jones touches upon this are characteristically confined to items that can be discussed with available experimental data. This is the more remarkable because few other subjects had received more speculative treatments in medical circles. Osler, to be sure, was very cautious but it was not until the 8th edition (1912) of his *Practice of Medicine* that the discussion of the etiology of gout acquired such definiteness as chemistry had provided.

Miller and Jones (1909) examined the Brugsch-Schittenhelm theory with the developing technique of enzyme manipulation. The theory was to the effect that a disturbance in the chain of enzymatic degradation of nucleic acids leads to an accumulation of uric acid. Miller and Jones did find that the organs of a patient (dying of nephritis) who had had gout were unable to destroy uric acid, but Wiechowski had shown that the organs of a gout-free man also lacked uricase. This decisive finding was confirmed by Miller and Jones. Other findings, particularly the absence of adenase in human organs, seems to have suggested to Jones the possibility of tracing some other anomaly of purine metabolism that might have a bearing on gout. The suggestion led to no systematic work on gout.

Leonard and Jones (1909) recognized that the developing knowledge of purine metabolism might account for exogenous uric acid and took cognizance of the developing problem of the

endogenous origins. Burion and Schur had found that perfused muscle produces uric acid at the cost of its hypoxanthine. Observing that the voluntary muscles of the pig, dog and rabbit cannot convert adenine to hypoxanthine, Leonard and Jones assumed that this ruled out nucleic acid as the source of the hypoxanthine known to be present in such muscles and then suggested that this set of data contributes to our knowledge of the source of endogenous uric acid. Voegtlin and Jones (1910) later perfused surviving muscle and found no alteration of adenine.

The production of uric acid in disease is touched upon again by Rohdé and Jones (1910) who found no difference between the enzymatic actions of normal and diseased rat organs. In this same paper endogenous uric acid is again touched upon.

In the article *On the Threefold Physiological Origin of Uric Acid* Jones reviewed the enzymatic studies up to 1910 with particular reference to the subject of gout. He indicates that uric acid may arise from the degradation of ingested nucleic acid, from the hypoxanthine (or its precursor) of the muscle, or through a *de novo* synthesis of the purine ring. Of course, it had been known for a long time that the latter takes place, for instance in starving salmon. Ascoli and Izar in 1908 had given a particular slant to the rôle of uric acid synthesis by claiming that uric acid is destroyed in tissues under aerobic conditions and is resynthesized from the products under anaerobic conditions. Calvery (1927), working in Jones's laboratory, could not confirm the findings of Ascoli and Izar.

Mention already has been made of the fact that gout had long been a subject of wild speculation. In view of this and the fact that Jones was frankly an enthusiast on the subject of purine metabolism one cannot but admire the restraint that he exhibits in his occasional writings on gout, a subject that still remains obscure.

STUDIES BEARING MORE SPECIFICALLY ON CONSTITUTION

As already stated, Jones had contributed to the view that only the residues of adenine and guanine and not those of xanthine and hypoxanthine are native to nucleic acids. An apparent

anomaly was presented by the case of guanylic acid which is now known to be a mononucleotide but which, in 1908, was still called a nucleic acid (see Jones and Rowntree, 1908). The products formed by the hydrolysis of guanylic acid had been in dispute and even the existence of the substance had been denied.

Jones and Rowntree (1908) were able to separate the well-established thymonucleic acid from the guanylic acid of the pancreas as did Levene and Mandal (1908) at the same time. Jones and Rowntree established the wide distribution of guanylic acid. They found guanine to be the only purine liberated on hydrolysis. In the same year Jones compared the nucleic acids of the thymus, spleen and pancreas, utilizing in his experimental work the means he had devised for freeing the preparations from guanylic acid. He gave additional evidence of the identity of this nucleic acid. It is noteworthy that in this paper, where there is resolved the confusion in the literature introduced by contaminating guanylic acid, this substance is spoken of as a "nucleic acid (if it be properly so called)". When Jones returned from further excursion in the enzyme field to the study of constitution, he recognized the general advance in the knowledge of constitution made by Levene and Jacobs and particularly their contribution to our knowledge of guanylic acid and he saw that guanylic acid is a mononucleotide (Jones, 1911) and yeast nucleic acid presumably a tetranucleotide (Jones and Richards, 1914).

Readily accepting the initial and decisive clarification of constitution that Levene and Jacobs had produced, Jones now turned his talents to some isolations projected by the new concepts of structure. From these it was reasonable to suppose that a residue of guanylic acid should be a component of yeast nucleic acid and that suitable hydrolysis of yeast nucleic acid should yield this guanylic acid. Jones in 1912 reported its isolation from a hydrolysate. Jones and Richards, 1914, reported a more detailed study of this substance. It was not emphasized that the constituents of a guanylic acid can be joined in different ways. Consequently, Jones's study of properties was not adequate to prove conclusively that the material isolated from yeast nucleic acid is identical with naturally occurring guanylic acid.

Evidence of the identity was shown later by Levene. Nevertheless, a milestone had been set.

In attempts to hydrolyze a nucleic acid by a partial cleavage that would leave dinucleotides, Jones and Germann (1916) tried Levene and Jacobs's method which consisted of heating the nucleic acid with ammonia at high temperatures. Jones and Germann found reasons for preferring lower temperatures. This and other hydrolytic processes were examined in some detail; stepwise oxidation by permanganate was also tried. By the latter means Jones and Kennedy in 1919 apparently removed the cytosine, uracil and guanine nucleotide groups of yeast nucleic acid and then they isolated for the first time in crystalline form adenine mononucleotide. Another milestone had been set. This isolation of adenine nucleotide had a three-fold significance. From a technical point of view it was an achievement. It joined with the isolation of guanylic acid in confirming the projected polynucleotide structure of yeast nucleic acid. Lastly, this nucleotide was the first of the group of adenine nucleotides to be studied and became the prototype of those compounds that now are seen to be central to several catalytic processes of the cell. Adenine nucleotide was described independently in the same year by Thannhäuser.

Thus there were identified in 1912 and 1919 two purine nucleotides stemming from yeast nucleic acid and giving concrete evidence of the polynucleotide structure of nucleic acid deduced from Levene's work.

In 1914 Jones and Richards reported the hydrolysis of yeast nucleic acid under conditions such that there were obtained "dinucleotides". This, surely, is a theoretical possibility. Its experimental pursuit was to be reported in several papers (Jones and Richards, 1915, Jones and Germann, 1916, Germann, 1916, Jones and Read, 1917, Read and Tottingham, 1917). In reviewing this subject in the second edition of *Nucleic Acids,* Jones remarks "without a single exception, every modern investigator in this field has prepared from yeast nucleic acid a substance which he believed to be a chemical individual containing more than one nucleotide group." While an examination of Jones's own reports leaves a good deal to be desired in the appli-

cation of criteria for individuality, it must be granted that the evidence found in his papers seems adequate to have supported the continuance of research. Where Jones erred was in using the assumed dinucleotides as material for the study of new problems before solving the basic problem of homogeneity. This criticism is so easy to write! Yet with these substances rigid tests have proved very difficult to apply,—so difficult that one wonders what would appear if neglected methods were to be applied to some of the materials reported even recently to be definite compounds. When Levene demonstrated that *certain* preparations of "dinucleotides" could be resolved into fractions each consisting of mononucleotides, Jones accepted this evidence that he had made a mistake in that particular instance, (see Jones and Abt, 1920). Was he *entirely* wrong in general? He implies that he thought dinucleotides to be real in the famous paper on constitution published with Perkins in 1923.

Jones had now entered a period when his genius for clarification had turned from long standing discrepancies in the literature to problems of constitution that were in the making. Mistakes were inevitable but no review of them is adequate without the realization that Jones reported fearlessly what he found and fearlessly admitted mistakes that he was convinced were such.

In 1916 Jones gave a presidential address before the Society of Biological Chemists. In the part that was published he notes that yeast nucleic acid when submitted to 5 per cent sulfuric acid at 100° C. liberates phosphoric acid as if from two sources —the purine nucleotides that release their phosphoric acid residues rapidly and the pyrimidine nucleotides that release their phosphoric acid residues very slowly. In this brief paper there is no very satisfactory analysis of the experimentally observed curve relating phosphoric acid liberated to the time of heating but in a subsequent series of papers individual nucleotides were examined and the results bore out the conclusion that there is a distinct difference in the ease of dephosphorylation of the purine and pyrimidine nucleotides. More exact measurements on rates were to be made later by Levene and Yamagawa. The method was to prove very useful. There is some confusion in following Jones's application of the method because he used

it in dealing with what he supposed to be dinucleotides, and in arguing on modes of linkage. If the central theme be reconstructed with the elimination of these special arguments it reduces to the following. The rate of dephosphorylation of yeast nucleic acid indicates two steps, the first *corresponding to* the rapid dephosphorylation of *isolated* purine nucleotides and the second to the slow dephosphorylation of *isolated* pyrimidine nucleotides. Jones does not deal clearly with possible modifications of rate that might be assumed if some of the phosphoric acid residues, which are terminal in nucleotides, are linked in the nucleic acid itself, although he stated in his 1922 paper on the thermostable agent of pig's pancreas that he had given consideration to this. He leads one to infer that he considered his data proof of the terminal position of the phosphate residues in nucleic acids. This sometimes obscures appreciation of Jones's finding that equal quantities of phosphate are set free in each of the two distinct steps. This latter fact remains one of the outstanding pieces of presumptive evidence that there are equal proportions of pyrimidine and purine nucleotide residues in nucleic acid. In the absence of precise quantitative analytical methods the physico-chemical evidence takes the lead. The method when applied to the elucidation of the modes of linkage led Jones to suggestive but inherently weak arguments.

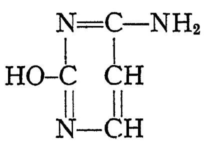
His next argument on structure utilizes some data on the number of free acid groups in yeast nucleic acid—data which Levene promptly criticized.

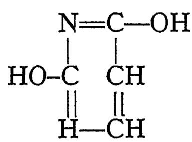
In 1923 (Jones and Perkins, 1923) Jones protested that he had never defended a formula for the mode of linkage of the nucleotides in yeast nucleic acid, having confined his efforts to "attacks on formulas proposed by others". "Now we change from the offensive to the defensive and propose the following formula." With respect to modes of linkage of nucleotides, with which alone Jones was then concerned, his formula differed from that of Levene (which is now copied in texts) by showing one ether linkage between carbohydrate and carbohydrate. I have established that the evidences in this paper were there *summarized* only after Jones had repeated the experiments again and again. Possible misinterpretations of the observations are

difficult to trace because in neither this nor other related papers are the protocols sufficiently detailed for this purpose. With these remarks I must leave detailed criticism of Jones's evidences to those more familiar with the field. None of the evidences which I happen to have seen for the modes of linkage of nucleotides in nucleic acid appeals to me as rigid proof or more than presumptive and since the evidence before me leaves contradictions that I would not presume to discuss without additional data it would be improper for me to defend or oppose Jones's conclusions.

Perhaps new evidence that certain nucleic acids are large polymers of nucleotides will force reconsideration of data that hitherto have been interpreted under the presumption of a tetranucleotide constitution.

In one of the last of Jones's publications he turns to the question of whether there is a parallelism between the deamination of purines and the deamination of pyrimidines during treatments of nucleic acids. The cytosine and uracil reported in yeast nucleic acid stand in the following relationship.


Cytosine


Uracil

Jones and Perkins (1925), on hydrolysis of yeast nucleic acid with 1 per cent NaOH solution, failed to find the uracil nucleotide. They say: "In so far as this result is of value the conclusion is obvious that the oxypyrimidine derivatives (uracil, uracil nucleoside, and uracil nucleotide) are not referable to an oxypyrimidine group in nucleic acid but are secondary products formed during hydrolysis by deaminization of the corresponding cytosine derivatives or their precursors."

Levene and Bass (1931, p. 276) refer to this paper as reviving the trinucleotide theory of yeast nucleic acid. If so, it was by inference, for no statement to that effect occurs in the paper.

Later Jones and Calvery (1927) discovered that the "failure

of Jones and Perkins and of Calvery to isolate uracil nucleotide from the hydrolytic products of yeast nucleic acid was due to a loss of material". By use of ammonia hydrolysis they obtained an hydrolysate from which all of the expected nucleotides were isolated.

It should not be overlooked that the failure to find uracil had been a *fact* that stood not only against accepted views but also against a group of evidences to which Jones himself had contributed. The publication might be said to have been premature but it cannot be denied that it was courageous. The withdrawal replaced Jones in the rôle in which he was supreme—the resolver of discrepancies—this time one originating in his own work.

NOTES ON MISCELLANEOUS PARTS OF JONES'S WORK

There were four papers published with Gamgee on the optical activity of nucleic acid and nucleoproteins. These papers were published in 1903. They bear internal evidence of Gamgee's authorship but that Gamgee was the author this writer has no proof. Aside from an abundance of entertaining remarks the papers present the then interesting observations that the nucleoproteins are unlike most proteins other than hemoglobin in that they are dextrorotatory, that the specific rotation of nucleic acid differs from that of the nucleoprotein, and that the specific rotations vary with the acidity of the solutions. Later Jones (1908) was to use the identity of specific rotations under fixed conditions and the uniformity of their variations with changes of the acidity of the solutions as an argument that the nucleic acids of the thymus, spleen and pancreas are identical. The optical properties of solutions of nucleic acids were to be referred to occasionally as, for example, by Amberg and Jones (1911), but were not extensively used. The change of rotation with change of the solution, especially its acidity, was a very important observation.

Since Jones was associated with Kossel at a time when the separation and analytical determinations of purines and pyrimidines were developing and since most early methods of sepa-

rating nucleic acids were empirical and their improvements dependent upon accretions of experience, it is difficult to appraise the originality of some of Jones's contributions to preparation and analyses. These aspects are so seldom emphasized in Jones's papers as to lead one to believe that, while he contributed his part, he would not have claimed a large portion of credit. In a letter to C. A. Morrow he writes:

"I do not know whether or not the description given in my monograph for the preparation of guanine and adenine from yeast nucleic acid is original. I have never seen it described in just this form anywhere. But you have to take into consideration that the preparation of guanine and adenine is very much more difficult with animal nucleic acid than with plant nucleic acid and the earlier descriptions for the preparation of the two bases applied to the more difficult preparation from thymus nucleic acid."

The two papers on phosphorus determination (1916, 1923) may be regarded as indulgencies. It is often said of a good analyst that he can do better with a poor method than a poor analyst can do with a good method. If Jones preferred to dust a dried precipitate of ammonium magnesium phosphate from the filter paper and discard the paper rather than to ignite and convert the phosphate to pyrophosphate it was doubtless his privilege and it was evidently his joy to show a reliability consonant with his own requirements.

In the closing years of Jones's career he was entering a field for which he was eminently suited—the examination of tissues for new material related to the compounds of his earlier studies. Mention has already been made of his studies of the naturally occurring guanylic acid, and of his interest in inosinic acid. Had space permitted we would have discussed his occasional concern with "β-nucleoproteins," substances or mixtures some of which had yielded guanylic acid. In 1922 Jones and Perkins turned their attention to those nucleotides in animal tissues that differ from the nucleotides of "animal" nucleic acid in having the pentose of "plant" nucleic acid. They say: "We were formerly inclined to believe that the presence of plant nucleotides in animal tissues is caused by the plant food which the animal consumes. But the tentative and confessedly inadequate evidence upon

which this view was based has since been found erroneous." Jones and Perkins then recovered from the "β-nucleoprotein" of the pancreas not only guanine nucleotide but also cytosine and adenine nucleotides that bore every resemblance to the corresponding *ribo*-nucleotides from yeast nucleic acids. The inference was that a *ribo*-nucleic acid is native to the animal, a view later confirmed by Jorpes. Said Jones: "It thus seems more than probable that the distinction between animal and plant nucleic acid will in the future not be so definitely drawn." Up to that time the distinction had been drawn sharply.

The reviving spirit of exploration that radiated from Walter Jones was made evident by unpublished examinations of corn and wheat "germ flours" and tubercle bacilli, by the note of Shaffer, Folkoff and S. Bayne-Jones on the nucleic acids of bacteria, by Calvery's examination of tea leaves, etc. Important contributions from Jones's laboratory were Buell's isolation of an oxyadenine from blood and her evidence that inosinic acid of muscle can originate in the adenine nucleotide.

Throughout his career Walter Jones counseled his assistants to pursue their own problems; but of the later period it may be said that he could not hold them from enthusiastic participation in cooperative and independent researches within his field. Of course, it is impossible to say what might have come out of his laboratory had not his illness cut short his own work and finally altered his staff. Nevertheless, it is clear that hands were getting close to remarkable substances that it was given to others to discover. Let imagination play with the dream of Walter Jones's enthusiasm could he now see that the materials which his hands almost touched are linking hitherto unrelated realms of research —catalysis of phosporylation, catalysis of oxidation-reduction, vitamins of the B group, also the structure of chromosomes. Again the pursuit of understanding for its own sake, from which Walter Jones would not deviate, bids fair to place in the hands of the physician more power than frontal attacks have yielded.

Jones's Monograph, *Nucleic Acids,* the first edition of which was published in 1914 and the second in 1920, remained for many years the only comprehensive review available in English. It dealt briefly with the historical background and set forth the

chemistry of the then known components of nucleic acids. Structures were discussed. Properly, a good deal of attention was given to the enzymatic transformations which Jones had studied extensively. The appendix gave invaluable directions for important preparations and there was a good bibliography.

Arthur Harden, reviewing the second edition in 1921, wrote "To the biochemist this book cannot fail to be of profound interest, alike for the importance of the matter and the lucidity of the exposition." With me Walter Jones left the impression that while he remained modest in his claims for the monograph he considered its consolidation of knowledge his duty as a scholar.

From time to time Jones contributed to various books and reviews.

Professor Abel had written several of the reviews of physiological chemistry for Gould's American Year-Book of Medicine and Surgery and from 1900 to 1905 Walter Jones and Reid Hunt together took over this laborious duty. These reviews were occasionally punctuated with spicy remarks. Examples:

"Morner's discovery of two isomeric forms of cystin as hydrolytic products of horn may furnish food for reflection to the artists who are accustomed to draw pictures of the proteid molecule without giving the sulphur atom any consideration."

"By a curious mixture of good chemic argument and unwarranted assumption, the authors (Nencki and Zaleski) arrive at an appalling structure formula for hemin."

"The discovery of an analytic method always marks an epoch in chemical development. . . . Fischer now introduces a method of separating the hydrolytic products of proteids . . ." (Fischer's famous ester method described at length).

The yearly comments on the developing knowledge of epinephrine, in which Abel kept the lead, are especially interesting and historically important.

The following bibliography of Walter Jones's scientific work is, I believe, complete except possibly the list of minor notices. Its order conforms in sequence to the numbering discovered in Jones's own, incomplete set of reprints and it contains titles not found in Jones's list of his papers,—the only document pertaining to his scientific work that was found among his effects after his death.

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Henry Mitchell

NATIONAL ACADEMY OF SCIENCES
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BIOGRAPHICAL MEMOIR

OF

HENRY MITCHELL

1830–1902

BY

H. A. MARMER

PRESENTED TO THE ACADEMY AT THE ANNUAL MEETING, 1938

HENRY MITCHELL

1830–1902

BY H. A. MARMER

Henry Mitchell was born on Nantucket Island, Mass., on September 16, 1830, the fourth son in a family of four sons and six daughters of William and Lydia (Coleman) Mitchell. The family was of Quaker stock long settled in America. His mother had been a school teacher prior to her marriage. His father, too, had been a teacher, but later became a bank official and one of the Overseers of Harvard College. Interested especially in astronomy, the father enjoyed a wide acquaintance among American men of science.

At this time Nantucket ranked after Boston and Salem as the third commercial city of the State, its prosperity being due primarily to the whaling industry. Nantucket whalers were ranging the seas from the Arctic to the Antarctic, and this fostered in the island community the study of navigation, including mathematics and astronomy. Henry Mitchell's mother used to relate that in her infancy the little children were taught to box the compass in the "Monthly Meeting School" in place of the catechism.

Young Mitchell obtained his education in private schools and at home where his immediate family furnished good examples and excellent instructors. The well-known astronomer Maria Mitchell was his sister, twelve years his senior. During his youth she was librarian of the Nantucket Athenaeum and was busily engaged in her astronomical labors, sharing her father's enthusiasm for astronomy. Under such conditions Henry Mitchell received a sound training.

In 1849, being then nineteen years of age, he entered the United States Coast Survey. His first assignment was in connection with triangulation, but later he was transferred to the hydrographic branch. Here his abilities soon became manifest. In 1854 he was entrusted with carrying out a tidal survey of Nantucket and Vineyard Sounds. This involved simultaneous observations at different places along the outer and inner

coasts to determine the progress of the tide from the open sea. Difficulties were experienced in the construction of tide gages to withstand the force of the currents and of the breakers at the outside stations. Mitchell was successful in devising a special form of tide gage which could be used on the open sea coast and in situations exposed to strong currents. This he described and illustrated in the appendix to the "Report of the Superintendent of the U. S. Coast Survey" for 1854.

For the following two years he was engaged in studying the complicated system of tides and currents in Nantucket and Vineyard Sounds and in elucidating their movements. In appendices to the Coast Survey Reports for 1856 and 1857, he sketches briefly the salient features of the tides and currents in these waterways. In the latter report, too, he describes a spar form of tide gage for observing tides in deep water and in situations exposed to heavy seas, that he used during the year.

In 1856 he began an investigation of the tides and currents in New York Harbor and vicinity. In this investigation Mitchell had two aims in view: first, the securing of data which would permit the prediction of the tides and currents in the localities in question for the use of the mariner; second, the study of tides and currents as agents of geologic change in molding and changing shore lines and harbors.

In New York Harbor, the tidal phenomena are complicated by the fact that the tide enters the harbor by two channels, one past Sandy Hook, and the other through Long Island Sound. Due to this, very strong currents are brought about in the East River, especially in the vicinity of Hell Gate, with very rapid changes in the time of tide through that waterway.

For studying the subsurface currents, Mitchell devised a simple and ingenious apparatus. To quote his own words "This consists of two large copper globes, as floats, connected by a slender cord, one weighted so as to float when immersed to the depth of four feet, and the other so as to sink to different depths in the currents which it may be desired to investigate. The motion of the apparatus will depend, of course, upon the difference of movement at four feet, the nearly superficial current,

and below, so that, obtaining by the ordinary log the movement at the surface, that below becomes known by observing the motion of the apparatus itself."

The investigation and elucidation of the tidal phenomena in New York Harbor was Mitchell's principal work up to and including the year 1859. Interim reports on the results of these investigations were published in the annual reports of the Coast Survey each year and in the 1859 Report he published an appendix on "The Physical Survey of New York Harbor and the Coast of Long Island, with Descriptions of Apparatus for Observing Currents." In this appendix he summarized briefly the relations existing between the currents and shoals in this region, and described several different forms of apparatus for measuring currents and also a new shape devised for a pile to be used as a support for a tide gage.

Beginning with 1860, Mitchell's attention was turned to the problems involved in the maintenance of the channels of Boston Harbor. In the Coast Survey Report for 1860 he published a discussion of the tides and currents in Boston Harbor. In the same Report he also described two instruments he devised for collecting bottom specimens in alluvial harbors.

Hydrographic surveys and studies of tides and currents along the New England coast and in New York Harbor engaged his attention up to 1866. In 1862 he also made a survey of Oregon and Hatteras Inlets, North Carolina, at the request of the military authorities in connection with the work of the North Atlantic blockading squadron. And in 1864, at the request of the Navy Department, he studied the action of floating ice in Delaware Bay.

In 1866 he enlarged his experience of hydrography to include the southeastern coast of the United States. In connection with a proposed cable uniting the United States and Cuba, he made soundings across the Straits of Florida, embodying the results of his observations in an appendix to the Coast Survey Report for the above-mentioned year. In this Report he also dealt with the difficulties in the way of laying the proposed cable. This work was continued during the following two years, the project

being enlarged to secure data beyond the immediate needs for cable laying so as to derive a better knowledge of the Gulf Stream.

At this time it was thought that a polar countercurrent existed below the northerly setting Gulf Stream, the low temperature of the deeper waters appearing to confirm this theory. Mitchell himself had accepted this polar current theory in a paper presented before the National Academy of Sciences in 1866. But in the Coast Survey Report for 1867 he challenges this theory, stressing the fact that between Cuba and Florida *"the Gulf Stream has a nearly uniform velocity, and constant course for a depth of six hundred fathoms, although its temperature varies in this depth 40° Fahrenheit."*

The growth of commerce, coupled with the increased draft and size of ships, was at this time bringing the problems connected with harbor improvement to the fore. Desiring to learn the latest developments in European practice, it was decided to send an American engineer to Europe. Although still a comparatively young man at this time, Mitchell was already recognized as an authority in hydrographic matters, and it was therefore natural that he should be chosen. Leaving in May of 1868, he visited Germany, Holland, England, France and Italy, studying canals and harbors and conferring with leading engineers. He also visited Egypt for the purpose of inspecting the Suez Canal which was nearing completion.

In February of 1869 he returned to the United States, and in the *North American Review* for October of that year he published a paper on "The Coast of Egypt and the Suez Canal." In this paper of some thirty odd pages there is not only an engineer's description of a great engineering undertaking, but also the reflections of a scientist on the hydrographic features of a region that posed a variety of problems relative to its development. The concluding paragraph of this paper furnishes a good example both of Mitchell's vigorous style and also of his interest in public matters outside the narrow specialized field of his own chosen profession.

"I have looked in vain through the entire history of this French enterprise in Egypt, to discover the least trace of

earnest effort or sincere co-operation on the part of the Egyptian or Turkish government. I believe that the Viceroys of Egypt, from Mahomet Ali down to the present weak prince, have been coaxed into acquiescence by the master minds that conceived and executed this brilliant work, and I am convinced that this costly avenue, and the commerce employing it, will never be secure from interruption till the territory is neutralized or otherwise wrested from Mohammedan misrule."

Another study resulting from his European investigations, and one of wider scope than the preceding, appeared as an appendix to the Coast Survey Report for 1869 under the title "On the Reclamation of Tide-Lands and Its Relation to Navigation." In this paper he formulates the relations existing between tidal and nontidal currents in regard to channel scour, and the effects of various types of reclamation works.

From his return in 1869 up to 1874 Mitchell was engaged principally with surveys and studies connected with the problems of New York Harbor and harbors along the New England coast. In the summer of 1870 he was also called to the Pacific coast to study the probable effects of extending piers in certain channels of San Francisco Bay. Interim reports on the progress of these studies appeared in the annual reports of the Coast Survey. In the Report for 1871 he also published a paper on "Hints and Suggestions Upon the Location of Harbor-Lines."

Early in 1874 he was appointed a member of the Commission on the construction of an Oceanic Ship Canal and made a personal inspection of suggested routes for the proposed canals through Nicaragua and the Isthmus of Darien. In that same year President Grant appointed him a member of the Board of Engineers to survey the mouth of the Mississippi River. The following year, at the request of James B. Eads, he served as a member of an Advisory Board in connection with the construction of the Mississippi Jetties. From 1875 to 1877 he also served on the Advisory Board to the Harbor Commissions of Virginia and Rhode Island, and in 1879 he was appointed a member of the Mississippi River Commission by President Hayes.

In connection with his work on tides, Mitchell studied the

question of the alleged emergence of the northeastern shores of the American Continent, this thesis being supported by eminent geologists. In a paper entitled "Notes Concerning Alleged Changes in the Relative Elevations of Land and Sea" in the Coast Survey Report for 1877, he made a critical study of the records upon which this thesis was based, and showed conclusively that it was untenable.

Delaware River, forming one of the important waterways of the country, presented numerous problems in connection with harbor improvement. In 1877 a hydrographic survey of this river was made under Mitchell's direction, and in the Report for 1879 he published an appendix under the title "On a Physical Survey of the Delaware River in Front of Philadelphia." Here he gave not only the results of the survey in detail, but also a general dissertation on channels in tidal rivers and the principles involved in the maintenance of such channels.

As a member of the Mississippi River Commission, he studied various problems connected with the Mississippi River. In the Coast Survey Report for 1882, he published a study on the effects of river bends in the lower Mississippi, in which he concluded that bends, on the whole, offer no advantages.

Surveys and studies of the harbors of New York and Philadelphia continued for the next few years, problems in connection with shoals becoming especially important. A careful study of Monomoy and its shoals by Mitchell appeared in the Coast Survey Report for 1886. And in that Report he also published an appendix "On the Circulation of the Sea Through New York Harbor" in which he called attention to the fact that "although the tidal currents of New York, especially in the East River, appear to move to and fro, with ebb and flood, in monotonous repetition, like the swing of a pendulum, there is a *net gain*, under ordinary conditions of river discharge, to the westward, *i.e.*, a permanent transfer of water from the Sound through the harbor and out into the ocean over Sandy Hook Bar." This was a very important discovery, both in elucidating the complex tidal movements in the harbor and in its practical applications in connection with the improvement of the harbor.

In May of 1888 he presented his resignation from the Coast Survey to take effect at the close of work on June 30, having served continuously for 39 years. During this time his attainments had received recognition in the world outside his chosen profession. Various scientific societies honored him by election to membership. In 1867 Harvard College conferred upon him the degree of Master of Arts; in 1869 he was offered the Professorship of Physical Hydrography at the Massachusetts Institute of Technology, and in 1873 the same chair was offered him in the Agassiz School of Science. In 1875 he was elected to the National Academy of Sciences.

Mitchell was married three times. His first wife was Mary Dawes, of Boston, to whom he was married in his early twenties and who left him a widower after twelve years. In 1873, he was married to Margaret Hayward who died in 1875, about five months after the birth of a daughter, his only child. Two years later, he married his deceased wife's elder sister, Mary Hayward. On resigning from the Coast Survey, he led a retired, studious life, spending his summers in Nantucket and his winters near Boston. In 1890, he served as a member of the Commission of the Annual Assay of the Mint; in 1893, he was appointed a member of the Advisory Council of the World's Columbian Water Commerce Congress; and in 1896, he published in the *Proceedings* of the American Academy of Arts and Sciences a biographical sketch of Ferdinand de Lesseps.

A year after his resignation he was offered the Superintendency of the Coast Survey by President Harrison. But Mitchell's health did not permit him to assume the burdens of the office, and he declined the offer. In March 1902 his wife died, following which he made his home with his daughter in New York City, where he died, December 2, 1902.

By a person who knew him in his later life he was described as "a charming and interesting man who shared the characteristics of his sister Maria in an ability to interest his friends wherever he went, conversing with great ease, and much ability to see the humorous side of life. He continued to dress in the style of his time, changing but little with fashion."

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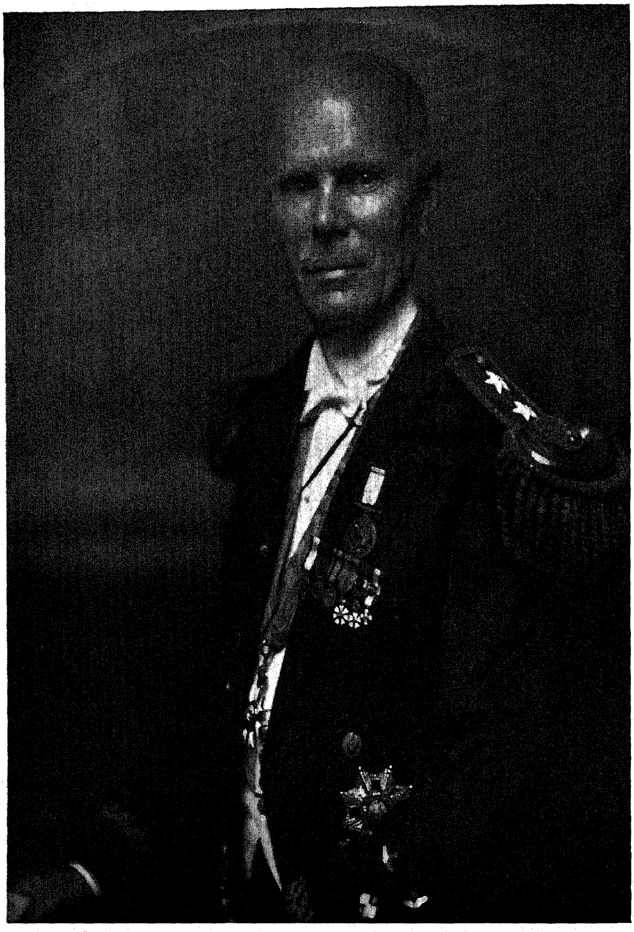

George O. Squier

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BY ARTHUR E. KENNELLY

George Owen Squier was a remarkable combination of American soldier, applied scientist, inventor, and engineer, as well as an army administrator and an outstanding Chief Signal Officer.

He was born at Dryden, Michigan, March 21, 1865. His parents were Almon Justice and Emily Gardner Squier. He entered West Point Military Academy at the age of eighteen, and passed through his four years cadetship training with distinction, graduating in 1887. In after years he used to say that at one time in his West Point career he accidentally fell half an hour behind in the routine of his studies, and that it took all his efforts during the remainder of his course to catch up with the schedule. He held the West Point course in high esteem and regarded the incident as an index of its precision. He often told interesting anecdotes of West Point cadet life, illustrating the *esprit de corps* which the institution develops among United States Army officers.

On graduating from West Point in 1887 Squier was appointed a Second Lieutenant in the 3rd Artillery Corps. His training in that branch of the service showed him the importance of accurate scientific knowledge in ballistics and ordnance engineering. He, therefore, took up the study of those subjects, by entering Johns Hopkins University at Baltimore as a graduate student, specializing in mathematics, physics, and ballistics. He became a fellow of Johns Hopkins in the academic year 1891-1892, and received there his Ph.D. degree in 1893, his graduating thesis being on the subject of chemical effects due to magnetism.

He was then appointed a First Lieutenant in the 3rd Artillery Corps and an ordnance instructor at the United States Artillery School in Fortress Monroe, Virginia. At this school he developed instruments for measuring the recoil of guns in action and the velocities of their projectiles. These researches were embodied in several papers and in a book written jointly with Dr. Albert Cushing Crehore—"The Polarizing Photo-chronograph".

At the outbreak of the War with Spain in 1898 Dr. Squier

sought service in the Signal Officer Volunteers and entered with the grade of Captain. In this service he was sent to the Philippine Archipelago, in 1900, where he commanded the cable ship *"Burnside,"* and laid a system of submarine cables between strategic points in the islands. He rightly concluded that the number of infantry men required to maintain effective garrison control over the islands could be greatly reduced by an extensive network of cable and wire communication, terminating in army headquarters. After the war he was appointed to the United States Signal Corps, first as Captain, later as Major, and finally as Chief Signal Officer in the California district.

It was during this period that he took up the study of army cable and radio communication, publishing several papers in this field. Major Squier discovered that a growing tree could serve as a receiving radio antenna if a nail were driven into it fairly high up and a wire brought down from the nail to the receiving instrument on the ground. As a corollary to the proposition that trees and their branches have sufficient conductance to serve as radio antennas, he showed that forests, shrubs, and vegetation generally act as partially absorbent media for radio waves passing over forest land areas.

He also made a study of aviation, then in its early stages of development. In 1908, Major Squier was the first passenger taken up into the air by the aviation pioneer, Orville Wright, in the latter's early form of airplane at Fort Myer. Twenty years later, the two men met in Washington to compare their aviation experiences; the first passenger thus comparing notes with the world's first aviator.

From the earliest days of the Wright brothers' flying machine Squier recognized the immense military importance of the airplane. A large part of his work as Chief Signal Officer was directed toward improving the range, power and effectiveness of this arm of the service as a separate branch of the military art. He succeeded in bringing the American military airplane into the front line of effectiveness during the World War. He foresaw that the bombing airplane would become a mighty engine of destruction in future wars.

In 1911 Squier was granted several United States patents for

transmitting telephone messages over cabled telephone conductors, using high frequency alternating current generation and the modulation of this impressed inaudible tone through the use of a microphone transmitter. This carrier frequency principle has since proved of great service in both wire and wireless telephony. Squier gave to it the name of *"wired-wireless"*. This invention added greatly to his fame as a scientist and engineer. General Squier also contributed a number of inventions for military service, notably a *"quick-aid" kit* for Army and Red Cross first-aid work.

From 1912 to 1916, Lieutenant Colonel Squier was a military attaché to the United States Embassy at London, where he made a special study of European military aviation and where the British Army authorities gave him special facilities for investigation. He was a close observer of the British technical radio and aviation activities during the first two years of the World War. He furnished an extensive report to the United States War Department of these activities. The United States Ambassador to Great Britain at that time, Walter H. Page, wrote a glowing account in his memoirs of Colonel Squier's services in London. Recalled to America in May, 1916, Squier was put in charge of the United States Signal Service as Chief Signal Officer. He organized and administered the electrical communication service between the American Expeditionary Force in Europe and its base in America, using for that purpose electrical communication of all types by radio, cable, and wires. This service continued until two years after the war. He was raised to the rank of Brigadier General in 1917, and from May 20, 1916, to May 20, 1918, was in charge of the Army Air Service, later receiving the distinguished service medal (D. S. M.) for his services.

General Squier was technical adviser to the American delegation at the International Conference on Electrical Communications in Washington during 1920. In 1921 he represented the State Department at sessions of the International Conference on Electrical Communications in Paris, and in the same year was an expert assistant to the American delegation at the Conference on Limitation of Armament, held in Washington.

General Squier was notable for his swiftness of judgment, resolute courage, and earnestness of purpose. His wiry, erect bearing and purposeful demeanor marked him at once as a military officer. He was ever punctual and precise in all engagements, while cheerfully putting late arrivals at their ease. He used to say that one of the many gifts of radio to the world was the radio announcer's habit of broadcast punctuality, even to the extent of ruthlessly cutting off a broadcast in the middle of a word. General Squier was a hard worker, facing every task with cheerfulness and courage. He never married, but was a family friend in numerous homes. With the aid of his sister, Mrs. Mary Squier Parker, who survived him, he built a "country club for country people" at his birthplace, Dryden, Michigan, where he succeeded in giving summer country associations to many of his friends and fellow townspeople. The Club has daily drawn hundreds of persons during the summer months, for recreation in games, boating, swimming, and other sports. This Club was one of General Squier's favorite hobbies. After his retirement from the Army in 1924, he frequently spent his winters in Florida and the other seasons in Washington and Dryden. Wherever he went, General Squier brought brightness and enjoyed popularity. His staff was enthusiastic in its praise and esteem for him.

Numerous honors were bestowed on General Squier both in this country and abroad. He was a Commander of the French Legion of Honor, a Knight Commander of St. Michael and St. George in Great Britain, a Commander of the Order of the Crown of Italy, and a member of the Royal Institution of Great Britain. General Squier held membership in the National Academy of Sciences, the American Philosophical Society, and was a fellow of Johns Hopkins University. He also received an honorary degree from Dartmouth College. General Squier was a life member and fellow of the American Institute of Electrical Engineers. He received from the Franklin Institute, the John Scott Medal in 1896, the Elliott Cresson Medal in 1912, and the Franklin Medal in 1919.

He died at Washington, March 24, 1934, at the age of 69.

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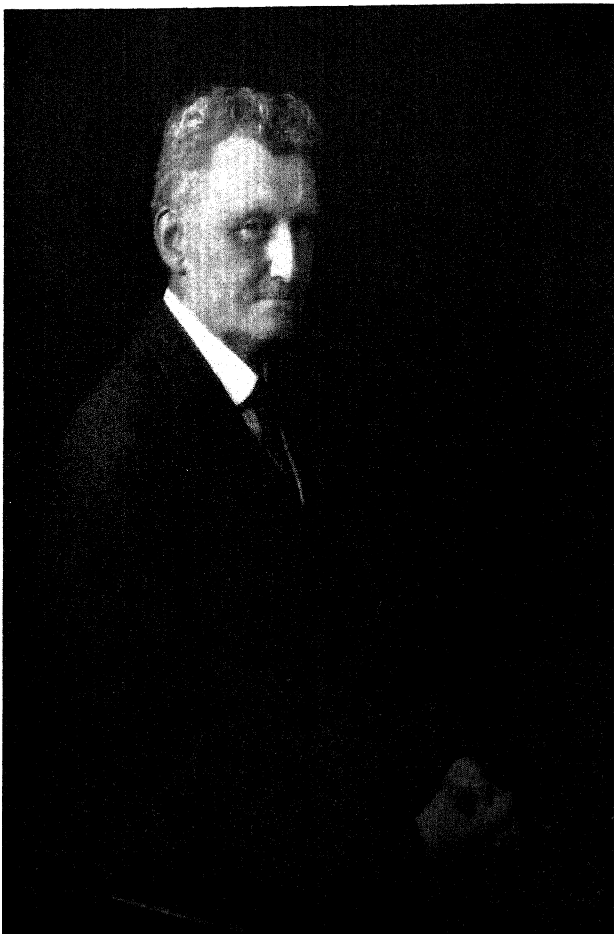

Geo. C. Comstock

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OF

GEORGE CARY COMSTOCK

1855–1934

BY

JOEL STEBBINS

PRESENTED TO THE ACADEMY AT THE ANNUAL MEETING, 1938

GEORGE CARY COMSTOCK

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BY JOEL STEBBINS

George Cary Comstock was born in Madison, Wisconsin, February 12, 1855, son of Charles Henry and Mercy (Bronson) Comstock. On his mother's side (Doan) his ancestry is traced to the Mayflower; on his father's side he was descended from William Comstock, who settled in Mystic, Connecticut, in 1628. William came from the town of Culmstock, on the river Culm, not far from Exeter, England. His grandfather moved from New England to Norwalk, Ohio, in 1810, and his father was a resident of Madison when the future astronomer was born. There were four children, three boys and a girl, of whom George was the oldest. One of the boys died in infancy; Caroline lived until about 1915; and Louis in 1938 is the Chairman of the Board of the New York Title Insurance Company, New York City. For business reasons the family had moved from Madison to Kenosha, Wisconsin, then to Sandusky, Ohio, and in 1869 they were living in Adrian, Michigan.

In the fall of that year George entered the high school, pursuing what was then known as the Latin-Scientific course. His natural tastes led him, with no advice from others, to select as far as rigid school curricula permitted, mathematics and physics. Quite unbeknown to his parents he was beginning to cherish an ambition to go to Annapolis. His school work was of such high quality that the superintendent of the school encouraged this ambition and brought him into contact with the Congressman of the district, who had the power of appointment on the basis of a competitive examination. In due course, toward the end of his senior year in high school, the examinations for appointment to the Naval Academy were held, and much was the surprise and satisfaction of the family when they learned that George's name headed the list. This was the first news they had had that George had even contemplated taking the examinations.

The appointment came in a few days, but his mother, having gotten over the first flush of joy and excitement, began to won-

der if after all the Navy was a good place for her son. The Civil War at that time had not receded very far into the background, and memories of the boys lost in the war were still green. After pondering over the matter, with some vigorous family discussions, she finally persuaded George to surrender the appointment upon her promise, backed by his father's assent, to give him four years at the University of Michigan. This decision rendered it necessary for the family to move to Ann Arbor so that George might live at home during his college days. The change to Ann Arbor was accomplished just before the beginning of the college year in 1873.

He matriculated that fall and became a candidate for the degree of Ph. B. Almost immediately he became acquainted somewhat as a friend and associate with two professors of mathematics, W. W. Beman and Edward Olney. George's classroom work caused him no worry or anxiety and he was privately consorting with the professors in discussions of advanced mathematics in the evenings. It was during one of these evening meetings with the staff that he was permitted to meet James C. Watson, Professor of Astronomy and Director of the Observatory. Watson at once took a fancy to Comstock, presumably because of his mathematical precocity, and it was Watson who suggested to him the study of astronomy. Into this new field Comstock entered with ardor and zeal.

It was in 1854 that the German astronomer, Francis Brünnow, had been called to Michigan. Trained in the traditions of his home institutions, Brünnow carried to a midwestern college the methods of a German university, and lectured in broken English to despairing and diminishing classes until Watson was his only student. Yet there was developed by Watson, who ultimately succeeded Brünnow, and the others at Michigan, the leading school for the study of astronomy in the country at that time. Of the astronomical graduates during or shortly before Comstock's time may be mentioned R. S. Woodward, Otto Klotz, C. L. Doolittle, and J. M. Schaeberle, and within about the decade following, W. W. Campbell, A. O. Leuschner, and W. J. Hussey. Comstock afterwards referred to Watson as the

cleverest astronomer he had ever met, but one who unfortunately had distributed his energies over too many fields.

Toward the end of his freshman year, following the panic of 1873, George saw the necessity of earning some money during the summers. Watson knew of the U. S. Lake Survey, then in progress on the Great Lakes under General Marr of the U. S. Army. Through Watson's influence George went to see General Marr with whom he concluded an arrangement to enter the service of the Survey, then in the Department of War. By the terms of this arrangement George spent six months in the summer, with leave for six months during the winter at the university. From then on his college course was shortened to six months each year, while he practically lived out of doors in a regular army camp for the remainder of the time. He took part as recorder and assistant engineer in the survey of Lakes Ontario, Erie, and Superior. His last year was on the upper part of the Mississippi River. In this work he became very expert in the use of the theodolite and level, an experience which was later to bring forth his text-book on field astronomy for engineers. During this same interval R. S. Woodward was also connected with the Lake Survey, but there is no record of the paths of the two young scientists crossing at this time.

George was graduated from the University of Michigan in 1877, and after an additional year on the Mississippi River and some further work at the observatory at Ann Arbor in connection with Schaeberle, he followed Watson to the University of Wisconsin late in 1879, to be assistant to the newly founded Washburn Observatory of which Watson was the first director. The scientific work of this observatory was scarcely started when Watson's premature death occurred in 1880, in only the second year of his residence at Madison. Edward S. Holden, later to become the first director of the Lick Observatory, took charge of the Washburn Observatory in 1881, and Comstock continued as assistant. During this period under Holden we find the first work of Comstock's in the Publications of the Washburn Observatory. Among the titles are: "A Catalogue of 195 Stars for 1880"; "A Table of Precessions in Right Ascension and Declination for 1880"; "On a New Method of Observing with the

Prime-Vertical Transit"; "Reduction of Observations Made By Two Observers for the Determination of the Latitude of Washburn Observatory by the Zenith Telescope"; "Determination of the Latitude of the Washburn Observatory by Transits of Stars over the Prime Vertical". It is seen that his activities were all in the astronomy of precision. Later under Holden's direction he did most of the work of preparing the "Tables for the Lick Observatory", which appeared in Volume I of the Lick Publications and have long been used at that institution.

Although Comstock was developing rapidly in his professional work, a career in astronomy involved considerable uncertainty, and he devoted his spare time to the study of law. He was graduated from the Wisconsin law school in 1883, and was admitted to the bar but he never practiced. Nevertheless, he later often referred to his legal training as possibly the most valuable part of his education. His precision of speech and his orderly habits were no doubt accentuated during his law studies.

At the age of thirty he was definitely committed to an academic career by an opening at Ohio State University, where he served as professor of mathematics for two years. He spent the summer of 1886 at the Lick Observatory where it was planned that he would take a position on the staff, but in 1887, when Holden left to take up active service at Mount Hamilton, it was President T. C. Chamberlin who called Comstock to take charge of the Washburn Observatory.

Throughout his scientific activity Comstock held an unusually happy balance between theory and practice. Though the observational astronomy of his early days consisted essentially of the visual measurement of angles, he never became a routine observer. The first work which he took up on assuming the directorship was novel in conception. As a substitute for the meridian circle and clock he placed a prism with reflecting surfaces in front of a telescope, and by observing simultaneously pairs of stars separated by arcs of approximately 120° the measures could be carried around the sphere in three steps, with the advantage that the quantities measured were small angles rather than large ones. From this work there resulted one of the best determinations of the constant of aberration made up to that time. Comstock's value

for this constant, $20''.44$, differed from the commonly accepted value of Struve by less than its own probable error.

The telescope used in the observations for aberration was Burnham's famous 6-inch refractor, which had been acquired by the Washburn Observatory in the early eighties. This instrument had been taken by Holden to Caroline Island in the South Pacific for the eclipse in 1883, and it was later used by Flint at the 1900 eclipse in North Carolina. The old wooden tube and mounting are in the museum of the Adler Planetarium in Chicago, but the objective with a new mounting is in current use at Madison.

A striking confirmation of the precision of Comstock's work was furnished some forty years later by Mr. Harry Raymond of the Dudley Observatory. In a discussion of star places for the Boss General Catalogue, Raymond found that Comstock's measures in the early nineties gave an excellent set of corrections to the system of star places then available. Expressed in equations these corrections took the form

$$\Delta\alpha_{\alpha} = -0^{s}.0048 \sin\alpha + 0^{s}.0063 \cos\alpha - 0^{s}.0015 \sin 2\alpha + 0^{s}.0048 \cos 2\alpha \quad \text{(Comstock)}$$

$$\Delta\alpha_{\alpha} = -0^{s}.0056 \sin\alpha + 0^{s}.0069 \cos\alpha + 0^{s}.0003 \sin 2\alpha + 0^{s}.0027 \cos 2\alpha \quad \text{(Raymond)}$$

Considering that Comstock's result was only a by-product of other work, the agreement of the respective terms of the two formulae is truly remarkable. Thus we have a modern appraisal of Comstock's skill, ingenuity, and precision.

Involved in the work on aberration was a determination of the atmospheric refraction, which decreases the apparent arc between any two stars in the sky. His measures established the effect of the relative humidity of the air upon the refraction and confirmed the superiority of the Pulkowa tables over the older ones of Bessel. His interest in the refraction was long continued and his retiring presidential address before the American Astronomical Society was entitled "The Atmospheric Refraction". This address was delivered in 1928, nearly forty years after his first published paper on the subject. One of his contentions was that

the effect of the air at low altitudes is not as uncertain as has often been supposed, and that other sources of error have been wrongly attributed to irregularities in the refraction. His simplified formula for the refraction,

$$R = \frac{983\, b}{460 + t} \tan z,$$

where R is the refraction in seconds of arc, b the height of the barometer in inches, and t the temperature in degrees Fahrenheit, gives the result within one or two seconds except under extreme conditions, an approximation sufficiently close for many kinds of work. This simplification of a complicated formula down to its lowest terms was typical of many of his contributions to practical astronomy. Additional terms and constants were devised for cases where greater accuracy was needed, but Comstock's formula for refraction will be remembered and used in its simplest form.

Of miscellaneous investigations extending over the years may be mentioned observations of minor planets and comets, discussion of the variation of latitude, occultations, especially during eclipses of the moon, physical observations of Mars, and a long series of micrometrical observations of Eros for the solar parallax during the opposition of 1900.

Concurrently with other investigations Comstock carried on measures of double stars with the 15-inch refractor for more than thirty years, from 1887, when he took over the directorship, to 1919 when he stopped definitely and collected all measures in a publication of the observatory. His observations were always of the highest quality, exemplifying the truth of the statement that "the precision of a double-star measure bears no direct relation to the size of the telescope with which it is made". He followed a number of interesting binaries and devised new methods of determining their orbits. His vice-presidential address before the Section of Mathematics and Astronomy of the American Association for the Advancement of Science in 1894 was on "Binary Stars". In fact, his interest in double stars was continuous throughout his active career.

Typical of his originality was his experiment on stellar color. By placing a grating of rods or coarse wires in front of the 15-inch objective, a series of spectral images was formed at the focus which was almost indistinguishable from ordinary stellar images. The measures of the separation of these spectra on either side of the primary image gave a numerical determination of the effective wave-length of the light of the star concerned. Thus astronomical colorimetry was placed on a quantitative basis. It was no doubt this interest in color which led him to point out the effect of differential atmospheric dispersion on measures of parallax when the objects concerned were of different spectra.

In fact, Comstock was continually attaching something different to one end or the other of his telescope. He devised a slat-screen apparatus for the meridian circle which reduced the image of a bright star to a multiple diffraction pattern, and this arrangement was used by Flint for many years in parallax and position observations.

Another new device was a double-image micrometer which was applied to the detection of the lunar atmosphere. Though not applicable to general micrometrical work, this instrument enabled him to observe the components of wide double stars close to the moon's rim. As no displacement was found up to the very instant of occultation of one star, he could set an upper limit to the negligible density of the moon's atmosphere.

A proposal by Comstock, the technical details of which he left to others to carry out, was the determination of radial velocity of stars by means of objective prisms. This and similar proposals by other astronomers have never worked out in practice, but the suggestions made by Comstock showed that he was alive to the problems of the so-called new astronomy.

The chief outcome of the double-star work was the detection of proper motions of faint stars. One high authority on double stars had stated that there was yet to be brought forth any evidence of the proper motion of a really faint star, but Comstock demonstrated that stars as faint as the twelfth magnitude do move enough to be detected. By the remeasurement of faint companions of bright double stars, observed incidentally by the

Struves and others early in the nineteenth century, he found that, when the known orbital and proper motions of the bright stars were allowed for, the remaining discrepancies were due to the motions of the faint stars. This conclusion was confirmed by a determination of the sun's way from the motions of the faint stars alone. In the work on proper motions he had the co-operation of Albert S. Flint, who determined many of the required modern positions of stars with the meridian circle at Madison.

Struve had found that for stars down to the tenth magnitude there was the empirical relation that the product of the magnitude and the proper motion was a constant, and Comstock extended this relation to the stars two magnitudes fainter. Thus he showed that the twelfth magnitude stars were nearer to us than would be inferred from their apparent brightness. He gave two alternatives: either there is an appreciable absorption of light in space or the stars which he studied are intrinsically fainter than the bright ones. The second alternative has turned out to be the correct one, and the great preponderance of stars of low intrinsic luminosity in a given volume of space, which his work foreshadowed, has been amply confirmed in recent years.

It was Comstock's determination of the proximity of faint stars that led him to the bold suggestion that the Milky Way is an absorption effect. We see farthest in the galactic plane where there is least absorption, while the stars appear fewest toward the galactic poles because their light is largely or totally blotted out in space. This speculation of course had to be abandoned, but it should be viewed in relation to what was current opinion in astronomy at the time. Newcomb had estimated the galaxy to be only ten or twenty thousand light-years across, and in the "Kapteyn Universe" the sun was placed not far from the center. The spiral nebulae still belonged to the galaxy; that they could be external systems of millions of stars had been considered and rejected by expert opinion at the beginning of the century.

The investigation of the aberration and refraction gave Comstock immediately a standing in the profession. When that work was published, appreciation came from various quarters,

notably from Loewy at Paris and Sir David Gill at the Cape, who wrote most friendly letters and discussed different possibilities of the new method.

In 1897 Simon Newcomb, owing to the age limit of the Navy, retired from the directorship of the Nautical Almanac office. Two years later this post was to become vacant again, and Newcomb urged Comstock to be a candidate for a professorship of mathematics in the Navy, with the understanding that he would become director of the Nautical Almanac. The receipt of such a letter as the following from the austere Newcomb must necessarily have been much appreciated.

Washington Jan. 5, 1899
1620 P St.

Dear Professor Comstock:

The post of Director of Nautical Almanac will be vacant next December by the retirement of Professor Harkness. It seems to me you are best available man for it.

Are you not willing to become a candidate for Professor in the Navy if you can look forward to the detail I have mentioned?

I hope you will be here at the proposed meeting of the committee on the Astronomical Society in February.

Very respectfully

S. Newcomb

This letter was followed by further correspondence, but Comstock preferred to remain in Madison.

Comstock was elected to the National Academy of Sciences in 1899, the first of the Wisconsin faculty to receive this honor. He was also a member of the American Academy of Arts and Sciences, and a life member of the Astronomische Gesellschaft.

In 1899 the Secretary of the Navy, John D. Long, appointed the first board of visitors to investigate and report on the conditions of the United States Naval Observatory. The board consisted of two members of Congress and three astronomers, Senator William E. Chandler, Representative A. G. Dayton, and Professors Edward C. Pickering, George E. Hale, and George C. Comstock. After thorough investigation and discussion, which included a canvass of opinion from the leading astronomers of the country, the board made a report which created a stir at

the time, but which was naturally not particularly welcomed by the Navy. The principal recommendation was that the astronomical work of what had become the national observatory should be placed under the direction of an astronomer rather than a naval officer. This reform, since repeatedly urged by scientific men of the country, was never carried out. The Navy has always been able to hang on to this fine place for the shore leave of a captain or rear admiral. Comstock was blamed or complimented for a leading share of the report, which unfortunately accomplished very little.

The American Astronomical Society grew out of the conference of astronomers and physicists held in connection with the dedication of the Yerkes Observatory in 1897. Comstock was one of the organizers of the society, and served for ten years as its first secretary. Later he held the office of vice-president, and in 1925 he was called from retirement to serve a term as president. He was always a prominent figure at the meetings, taking a leading part in the discussions, whether on business or on scientific questions. He was an admirable presiding officer, and he once remarked that it was the function of the administration to pick out and develop undiscovered talent among the younger men.

He was the chairman of the committee of the society appointed to coordinate the observations of Halley's comet in 1910. On the initiative of this committee an expedition in charge of Ellerman was sent to Hawaii to attempt the observation of the head of the comet which projected against the sun's disk. The report of the committee appeared in Volume 2 of the society's *Publications*.

It was during Comstock's term as president that the society was incorporated under the laws of the State of Illinois. The informal status of the organization had been repellant to his mind, and moreover it was just as well for the society to be in a position to receive donations without legal difficulties.

Throughout his career Comstock carried on instruction as well as research. The number of his students was never large, but he was known to those who came to him as a master of clear and apt expression. When the present writer was a gradu-

ate student he thought that Comstock was the best teacher he had ever had, a judgment which has changed little over the years. Yet in the ordinary sense Comstock did very little teaching for his advanced students. They went along much on their own until difficulties arose, and then his ability to elucidate obscure points would be shown. A student learned from him through inspiration and by imitation. He was a methodical observer and an expert computer, and one needed only to be around and watch him to get some of the intangibles which make for successful scientific work. An occasional phrase or sentence, such as "I believe in an astronomer's making his own instruments", was worth more than an hour of formal instruction.

Much of his success with students was due to his linguistic ability. For years the precision and elegance of his English were noted in the university community. He also was fluent in German, French, and Italian.

The relations with students naturally brought out several papers covering problems of instruction. He contributed notes on the adjustments of a sextant, on the establishment of a meridian line, on the graphical representation of a comet orbit, and on the motions of comets when far from the sun. He was an expert in time determinations with small instruments, and he showed that the precision attained with a 3-inch broken transit with a reversal of the instrument on each star was comparable with the best results of large meridian circles, a conclusion amply confirmed by modern experience in longitude determinations.

In the course of his teaching he also had occasion to write several text-books. The first appeared in 1890, a little work entitled "An Elementary Treatise upon the Method of Least Squares, with Numerical Examples of its Application". He boldly assumed without proof the fundamental equation of the law of errors, pointing out that after all the real justification of the method is that it agrees with experience. Though this little work is out of print, there is still no better place for the novice to look up the essentials of least squares, and how to proceed in a simple practical case.

The "Text-Book of Astronomy" was written in 1901 for

students of high school or junior college grade, and was accompanied by a manual with numerous suggestions for the teacher. Illustrative exercises with simple apparatus were proposed, as it was known that many teachers without previous training in astronomy were being called upon to give an elementary course in the subject.

For many years all of the students majoring as civil engineers at Wisconsin were required to take the course in practical astronomy. The attitude of the engineering faculty was that they were not so anxious to have the students learn astronomy as they were to have them get the unusual training in observation and computation under Professor Comstock. In his textbook of "Field Astronomy for Engineers", which appeared in 1903 with a second edition in 1908, he combined the sound instruction in tested methods of practical astronomy with new applications to the ordinary engineer's transit in the field. He showed that the determination of time, azimuth, latitude, and longitude with small instruments could be made much more precise than was ordinarily assumed. In 1919, as part of his war service, Comstock's experience in teaching navigation to prospective mariners led him to get out a little work on "The Sumner Line".

To a faculty member with Comstock's qualifications there naturally came many important university duties. He was chosen chairman of the committee of arrangements at the time of the Jubilee, the fiftieth anniversary of the founding of the University of Wisconsin. One of the important measures of the first year of the administration of President Van Hise at the university in 1904 was the definite organization of the graduate school. He selected Comstock to be the head of the school, and placed on him the task of working out the problems of a new division of the university, one that was growing rapidly both in size and in importance. He held this position until 1920, as chairman, director, and dean, showing in it his qualities of quiet efficiency and breadth of view. He received a school without definite organization and with about one hundred and fifty students; he left it fully organized for teaching and for research and with its number nearly quadrupled.

Early in his work in the graduate school Comstock once re-

marked humorously that he was somewhat handicapped in the making of Ph. D's. by his own lack of a doctor's degree. This defect was remedied in due time by the award of the honorary degree of LL.D. by the University of Illinois in June, 1907, and of Sc.D by the University of Michigan a week later on the thirtieth anniversary of his graduation.

The duties of the graduate school naturally interfered with his scientific work, but probably the most important of all his investigations, that on the proper motions of faint stars, was carried on amidst other duties of administration and instruction. On relinquishing the deanship he continued active for two years more before retiring from the observatory in 1922 at the age of sixty-seven. Although he could have served several years longer he decided to retire, and this decision like all others of his career was clean cut and final. He finished and published the researches on which he had been engaged, leaving no loose ends about to bother his successor.

Comstock was very fortunate in his family life. In 1894 he married Esther Cecile Everett of Madison who with their daughter Mary, now Mrs. George Carey, survives. The home on Observatory Hill was long known as a center of hospitality, especially to the graduate students, in whom the dean and his wife took a personal interest. Perhaps the explanation of his wide sympathies and interest in people and in current events was the fact that he left astronomy behind each day when he closed the observatory door. After his retirement from university service, Professor and Mrs. Comstock traveled around the world, renewing friendships with scientific colleagues in many countries; they returned to settle in Beloit, Wisconsin, just around the corner from the great attraction of three grandchildren. Here he spent the last dozen years of his life.

Despite his dignified or even austere manner Comstock had a keen sense of humor, which combined with a promptness of decision made him equal to any occasion. At the time of the appearance of Halley's comet in 1910, when he saw the popular interest that was impending, he arranged with the university authorities to make a small admission charge on some of the nights when the observatory would be open to the public for

viewing the comet. On one of the days during this rather hectic astronomical period he was called to the telephone by an inquiring taxpayer. The question was: "Professor, what are you going to do with the money you are collecting for a view of Halley's comet?" Promptly came the response: "Madam, we are going to get a new tail for Halley's comet." The reply seemed to be entirely satisfactory.

In his youth Comstock had been a serious individual with little aptitude for play or sports, but in mid-life he took up golf on the insistence of his family. In his later years he became an ardent member of the Rotary Club of Beloit, and was made an honorary life member of the organization. He had the pleasure of visiting and afterwards reporting on various Rotary Clubs in Europe. At home he was in constant demand as a speaker before service clubs, his topics ranging from club education programs and popular talks on astronomy to philosophical discussions of a more severe order.

He was fortunate in maintaining his physical and mental vigor up to the end. He gave a public address just two weeks before he died, and at the last he was ill for only a few days, being taken by an embolism following a minor operation. The end came quickly on May 11, 1934, in his eightieth year. As has been aptly said, there is always an old school in a progressive science. Comstock lived to become one of the old school in point of years, but his outlook was always forward. He saw the astronomy of his youth grow into the astrophysics of the present, but his conception of all science was like that of the heavens described in his own text-book, "A universe which is ever becoming something else and is never finished."

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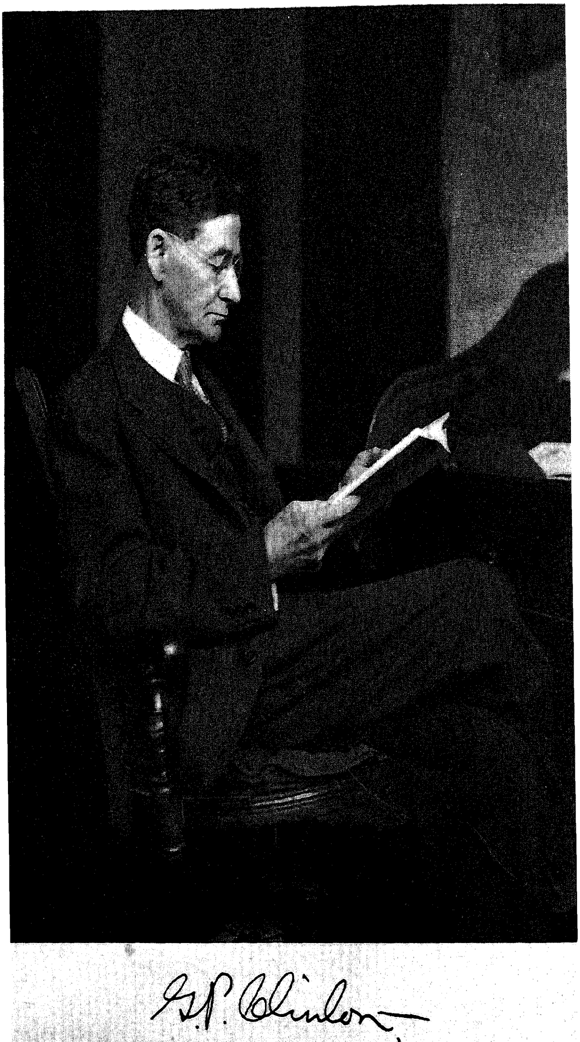
G. P. Clinton

NATIONAL ACADEMY OF SCIENCES
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OF

GEORGE PERKINS CLINTON

1867–1937

BY

CHARLES THOM AND E. M. EAST

PRESENTED TO THE ACADEMY AT THE ANNUAL MEETING, 1938

GEORGE PERKINS CLINTON

Chronology

1867. Born May 7, at Polo, Illinois.
1886. Graduated, Polo High School.
1890. B.S., University of Illinois. Assistant Botanist, Agricultural Experiment Station, and Assistant in Botany in the University of Illinois.
1894. M.S. in Botany.
1900. Graduate Student in Botany, Harvard.
1901. M.S., Harvard.
1902. D.Sc., Harvard.
1902. Botanist, Connecticut Agricultural Experiment Station at New Haven, July 1, 1902.
1902–1925. Botanist, State Board of Agriculture (Connecticut).
1903–1927. Chairman, Committee on Fungous Diseases of Connecticut Pomological Society.
1904. Agent and Expert, Office of Experiment Stations, U. S. Department of Agriculture, studying coffee diseases in Puerto Rico.
1906–1907. Studied fungous diseases of the brown-tail moth at Harvard, 1906. Continued the study in Japan, 1907.
1908. Agent, Bureau of Entomology, U. S. Department of Agriculture, to prevent the spread of moths.
1909. Collaborator, to seek parasites of the Gypsy moth in Japan.
1915–1927. Lecturer in Forest Pathology, Yale.
1916. Collaborator, Plant Disease Survey, U. S. Department of Agriculture.
1926–1929. Research Associate in Botany, Yale.
1937. Died, August 13th at New Haven, Connecticut.

GEORGE PERKINS CLINTON*

1867–1937

BY CHARLES THOM AND E. M. EAST

George Perkins Clinton was born in Polo, Illinois, May 7, 1867. He was known to his fellows as Botanist of the Connecticut Agricultural Experiment Station, a post which he held for thirty-five years. His background was an ancestry which had its roots among the founders of New York and New England. Direct progenitors on both sides of the family served in the American Revolution. He was the son of John Waterbury Clinton who was born in Andes, New York, but moved to Ogle County, Illinois, when a young man, as a teacher; there he married Caroline Perkins whose family had moved from Delhi, New York, to Buffalo Grove, Illinois. Two great-grandfathers, Joseph Clinton and Rufus Perkins, fought in the Revolutionary War; one grandfather, Timothy Perkins, in the War of 1812, and an uncle, Edgar Perkins, was in the Civil War. John Waterbury Clinton published the "Polo Ogle County Press" for nearly fifty years. He was interested in all the early movements toward popular education which resulted in the establishment of colleges throughout the state. As editor of a paper in a community predominantly interested in farming, he supported the development of the University of Illinois, and especially its agricultural work. He was a collector of coins, stamps, geological specimens, personal and historical material especially concerning the region in which he had settled. His house was full of books and magazines. His one hobby that may have turned his son toward botany, was his development of flower gardens. We have thus the background of a scholar.

One of his teachers writes that young Clinton was a "studious, hard-working pupil," "very much disgusted and impatient with himself if he made a mistake" and very ambitious to make a record for scholarship. There is no report of his first stimulus

* Dr. Florence A. McCormick, who was closely associated with Dr. Clinton during the later years of his work at New Haven, collected most of the materials used in this memoir.

to study plants but by the time he graduated from high school at Polo in 1886, he had acquired the beginnings of a collection of native species of the prairies, and an interest in botanical training which led him to seek information about the University of Illinois. This brought him into contact with M. B. Waite of Oregon, Illinois, not far from his home. Mr. Waite was already specializing in botany at the University and working in the field as assistant during the summer. Clinton entered the University in the fall of 1886.

The great figure in botany at the University of Illinois for many years was Prof. T. J. Burrill, who was about at the height of his career when Clinton was in college. In connection with The Illinois Natural History Survey, Burrill kept his special students and assistants collecting and classifying the flora of the state. Clinton was early assigned to the fungi and when he graduated in 1890, went to work for the University as an assistant. Preliminary publication upon two of the great groups of parasitic fungi had been made by others before Clinton graduated. The manuscript upon the smuts (Ustilagineae) was in process and fell to him. He published the smuts of Illinois while in the service of the University.

He remained with Dr. Burrill until 1900, working for the most part upon the fungi of the state. Fourteen publications are reported for this period. Those of us who knew him in the laboratory in the late 1890's and shared an acquaintance with him for the next forty years, know that he changed little in appearance during that time. Then as later, he was a slight man, serious, methodical, purposeful, chary of word unless he was sure of his audience but worthy of a hearing when he chose to speak.

One who saw much of him during his professional career reports that in spite of close and persistent application to the work on hand, "He was never too busy to help a comrade or a colleague, to the best of his ability; and his comments never failed to show a keen insight into the problem under discussion" and again, in thirty years of acquaintance he was not heard to

say, "A word about an acquaintance that could be called sharp, or mean or disagreeable."

The casual visitor found Clinton the formal, cautious scientist, sparing in the use of words but succinct in his discussion of the problem presented. Only when one got behind the scenes and found him jealously using and treasuring for thirty years the chair and desk inherited from Thaxter, he caught the strain of hero worship which put high value upon the ideas and ideals of his predecessor. Then in his memoir of that friend, we find the poetic spark which fired that elemental imagination which somehow lights the way for every constructive worker through long years of unremitting application to problems which are deadly to men who lack that inspiration.

The correspondence of those ten years in the Illinois Survey inevitably brought him into contact with Dr. Farlow at Harvard. This led to a visit to Cambridge, and in 1900 Clinton took leave from his University assistantship for graduate study with Professors Farlow and Thaxter. He took with him his Smuts of Illinois. His thesis extended it to the Smuts of North America and, like many another who fell under the spell of Thaxter—he carried away that group as a life task. Other assignments might come and go, the monographic work upon the smuts was still in process when he died. Fortunately he had arranged with associates for the completion of work in progress thus to insure that his revision of the Smuts of North America will be published.

He received the doctorate of science at Harvard in 1902, and became a member of the honorary society of Sigma Xi. The post of botanist at the Connecticut Agricultural Experiment Station at New Haven, once honored by Thaxter himself, was given to Clinton, July 1, 1902. He retired from it on July 1, 1937, after thirty-five years of distinguished service, and died August 13, 1937.

The record of those thirty-five years appears principally in the biennial reports of the "Station Botanist" and in the publications of the state societies interested in agriculture. Clinton's survey of the outstanding disease records of a year show how

close a watch he maintained upon the parasitic diseases of Connecticut crops. In addition, each such report shows one or more studies of particular diseases or of special fungi or fungous groups. Connecticut presents a wide range of conditions—forests, rolling hills, areas devoted to forage crops, intensive market gardening, tobacco as a special crop, apples, peaches on a commercial scale, ornamentals, and greenhouse products—one has but to read the topics in Clinton's reports to find that he was keenly alive to the problems of crop production throughout the state. No group was ignored or neglected.

The working plant pathologist consistently watched Clinton's reports for summaries of progress, for new methods, for observations and illustrations of the newly recognized disease or for the foreign invader. In each case the history and significance of the disease was presented together with the definite additional material developed by Clinton and his colleagues working at New Haven. These papers were illustrated by photographs and drawings chosen to convey to the grower as complete as possible a concept of the type of injury to crop plants attributable to the infecting agent. Many fungi were carried to the laboratory where life histories were worked out carefully. Among such special studies, we find Phytophthora of potatoes with special investigation of the production of oospores, Phytophthora of Lima beans, Thielavia as a root rot of tobacco, the bacterial disease—tobacco "wild fire," an elaborate series of papers upon the chestnut bark disease, another series upon white pine blister rust, a survey of the heteroecious rusts observed in the state.

He began work on what are now known as the virus diseases about 1903. In various forms, references to the group appear in each report. In 1914, experimental work on tobacco mosaic over a number of years was brought together in his well known paper—Calico of Tobacco. The whole matter again appears in his joint paper with Dr. McCormick in 1928.

Clinton was an active member of various societies covering the agricultural field in Connecticut. Few state meetings were held without his presence and participation. Aside from his

general reports, his name appears upon eighteen committee reports to the Connecticut Vegetable Growers' Association, upon twenty-four reports to the Connecticut Pomological Society, and as author or joint author of twenty Experiment Station bulletins. He interpreted his place of the Station Botanist as a service job. To the broad interest in plants and consecration to the interests of the state so typical of Burrill, he added the critical taxonomic point of view characteristic of Farlow and the persistent, patient descriptive ideals of Thaxter.

As a Connecticut scientist, in addition to the groups already mentioned, he was a member of the New England Botanical Society, the Connecticut Botanical Society, the Connecticut Forestry Association.

The merit of his contributions to agriculture and to botany was recognized early by his colleagues beyond the state service. Although he was modest and unassuming, he was constantly in demand for counsel and instruction. Harvard called him back once. Yale made use of him for a dozen years. The United States Department of Agriculture requisitioned his service from time to time and the Phytopathological Society made him its president in 1912. His election to the National Academy of Sciences was welcomed by all who knew him in 1930.

In addition, he was a Fellow of the American Association for the Advancement of Science, Fellow of the American Academy of Arts and Sciences since 1914, member of the Mycological Society of America, of the American Society of Plant Physiologists, of the American Naturalists and of the Botanical Society of America.

Clinton was a confirmed collector. He began building his personal herbarium as a boy at Polo, Illinois. Four years at the University added the plants of South Central Illinois; ten years on the Survey made him a discriminating collector whose critical eye caught host and parasite in the same glance. His herbarium grew from every journey and each assignment. Thus he added series from Canada, from the United States, from Puerto Rico, Panama, Hawaii, Japan and from parts of Europe reached on his trips abroad. Each expedition was a scouting trip—his wide

acquaintance with host plants and parasites made him alert to detect the spread of plant disease into new territory and to anticipate its significance to his own state.

His herbarium with the record of fifty years of field work, was given to the Connecticut Agricultural Experiment Station. The descriptive literature assembled by him remains with the collection. The two together will make that station a place of pilgrimage to the phytopathologist seeking to follow the story of the fight to save our crop plants from destructive pests.

Clinton married Anna J. Lightbody of Pekin, Illinois, on August 9, 1892. Their only son, Harry Lightbody Clinton, was killed in France, fighting with the American troops in the World War. This loss saddened the latter years of the father's life. He never lost his sense of acute grief. Work in increasing amounts was his only means of escape. While he retired voluntarily July 1, 1937, he retained the title of consulting botanist and went to work each day as usual, as long as he was able to get there. Mrs. Clinton continues to live in New Haven, Connecticut.

The annual loss to agriculture from plant diseases is variously estimated all the way from half a billion to three billion dollars. Whatever may be the true figure, the loss is great. The only protection of the public against such enemies, is a home guard, smaller than an army regiment in times of peace, composed of fighters armed with that knowledge of the character and habits of the enemy which comes from thorough training in plant pathology. One who occupied an outpost along this battle line was lost when George Perkins Clinton died August 13, 1937.

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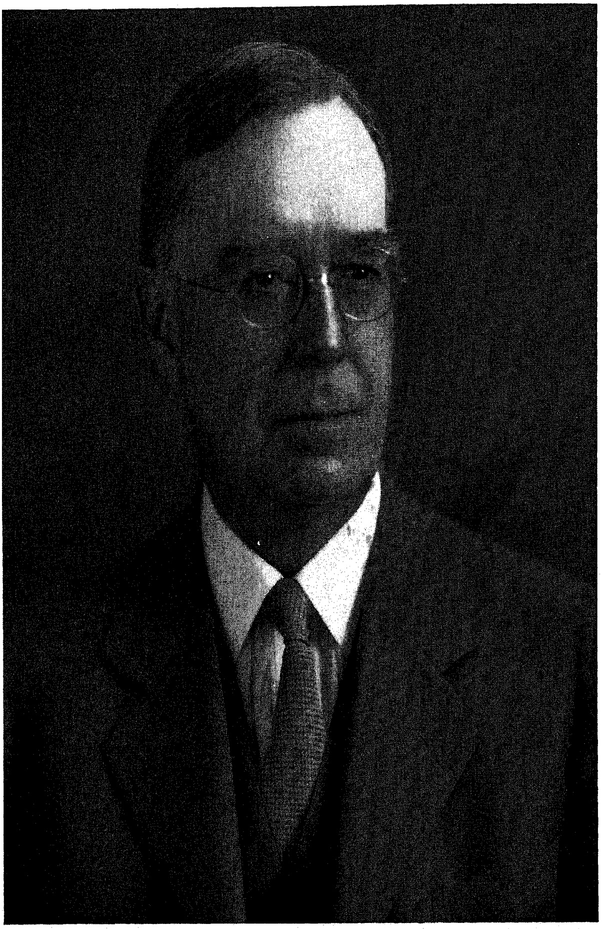

Edwin O. Jordan

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BY

WILLIAM BURROWS

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1866–1936

By WILLIAM BURROWS

Edwin Oakes Jordan was born July 28th, 1866, and died September 2nd, 1936. During his lifetime his chosen fields, bacteriology and public health, developed from their beginnings to their present outstanding position among the scientific activities of today. Although the initial contributions of Pasteur and Koch preceded Jordan's college days by a few years, the new knowledge was slow in reaching America in the form of actual practice and was beginning to come with increasing force when he started his college work. The effect of the heady stimulation of the new science of bacteriology on the young man, and the application to it of his own unusual abilities throughout the remainder of his life, go to make the development of the man and the scientist a brilliant facet of the history of the growth of bacteriology in this country.

Jordan was born at Thomaston, Maine. His grandfather, Oliver Jordan, was a master-mariner and shipowner who, after he retired from active life, lived in a dignified and beautiful home on Main Street, the street of fine houses in Thomaston. His father, Joshua Lane Jordan, was one of nine children and, like many others in his day, went to sea at an early age. By the time he was twenty-one he, too, was a captain and commanded first one and then another of the merchantmen then building at Bath and Thomaston.

Captain Joshua Jordan married his third wife, Eliza D. Bugbee, in 1865. When their first child, Edwin Oakes, was six months old, the father took command of the vessel, *Pride of the Port,* sailing from Thomaston. The family, augmented at Bombay by another child, did not return to this country for three years. Their home was, for the most part, the captain's cabin on board ship, with occasional brief stays ashore at such ports as Liverpool and Bombay. When Edwin Oakes was three and one-half years old, his father retired from seafaring, settled down in Thomaston in a house close to the grandfather's home, and engaged in the local banking business.

Mrs. Jordan was considerably younger than her husband. She was of Puritan stock which settled in this country early in the seventeenth century. After several years in the Normal Training School at Framingham, Massachusetts, she had taught in a country school. Although exceedingly shy and retiring, she was a woman of strong character, a devout Christian with a high sense of duty, with great potentialities for self cultivation and a saving sense of humor. She took a very active part in the education of her four children, interesting them both in good literature and in natural science. Jordan's self-effacing character, combined with an inner force which caused him to push forward any project that he felt to be good, without regard to the cost to his natural timidity, was undoubtedly derived directly from his mother. It was through her that he began a collection of minerals, an avocation that afforded great delight in his boyhood and a keen secondary interest throughout his life. During a number of his adolescent years he intended to become a mineralogist or geologist.

Thomaston was a small town in the seventies but a very busy and prosperous one with its shipbuilding, lime-quarrying and lime-burning industries. The life of the boy there was full of pleasant incidents, such as picnics upon land, mineralogical expeditions behind the pony, Nebuchadnezzar, and rowing and sailing a boat on the tidal river. Thomaston did not, however, present very good educational opportunities and on this account the family moved to Auburndale, Massachusetts, in 1881 where Jordan attended and graduated from the Newton High School. In 1884 he entered the Massachusetts Institute of Technology.

Already interested in natural science through his mother and having a nodding acquaintance with the methods of science through his collection of minerals, Jordan came in contact with William Thompson Sedgwick. Sedgwick exerted a strong influence on the young man, an influence whose impress remained with him throughout his life. This was evident not only from his continuous and lifelong devotion and loyalty to Sedgwick, but even more in his habits of thought and his marked tendency to think not only of the immediate consequences of experimental observations but, further, their relation to broad biological prin-

ciples. Undoubtedly this characteristic was innate in Jordan's own mind and very possibly his semi-philosophic attitude formed the nucleus of the close bond between him and Sedgwick.

Sedgwick was himself a young man, having been appointed Assistant Professor of Biology only the year before Jordan entered college. He became intensely interested in the newly born science of bacteriology and quickly grasped the significance of the new facts. The golden age of bacteriology was at its heyday in the eighties. New discoveries tumbled one after another with bewildering rapidity—the discoveries of the tubercle bacillus, the typhoid bacillus, the diphtheria bacillus, the meningococcus—and the giants of the day, Pasteur, Koch, Loeffler, Weichselbaum, Kitasato, Gaffky, Eberth, Pfeiffer, Roux and the rest, were not names only but active workers. Small wonder that those who took their first courses in biology with Sedgwick felt that a new world was opening up before their eyes. Jordan's active mind responded vigorously to this stimulation. It is much to his credit that he was not carried away in the whirl of medical discoveries but kept a solid footing on purely biological foundations. His interests followed those of Sedgwick into the sanitary and hygienic significance of the new knowledge, a field which remained of first importance to him all his life.

The year of Jordan's graduation, 1888, Sedgwick was appointed Consulting Biologist to the newly organized Massachusetts State Board of Health. Through Sedgwick, he was appointed Chief Assistant Biologist at the Lawrence Experiment Station and was intimately associated with the early experimental work carried out there on sewage and on the filtration of water. Before beginning the work at Lawrence, however, he spent two months with T. Mitchell Prudden at the College of Physicians and Surgeons in New York. Prudden, stimulated by the current discoveries in bacteriology, had gone back to Germany and spent a month in 1885 studying under Koch and had returned to America with the latest information. From him Jordan obtained first hand information on the differentiation of the typhoid and colon bacilli, the use of agar in semi-solid media, the Gram and Ziehl-Nielsen stains and other technics in use in Koch's laboratories. Something in Prudden's reticent but rich personality

appealed strongly to Jordan and the former's influence did much to augment the interest aroused by Sedgwick.

For the next two years, until 1890, Jordan carried on an intensive experimental study of the bacteria present in water and sewage. His efforts were devoted for the most part to the application of Koch's semi-solid gelatin medium to the study of flora characteristic of water and sewage. In later years he often spoke of the tedious methods used in the preparation of plates—the mixture of gelatin and water sample was poured on chilled rectangular plates of window glass and, after hardening, these were placed in tiers in shallow covered glass jars. Liquefying colonies, melted plates and other hazards made counting difficult and on a number of occasions in later years he remarked with solemn visage but a twinkle in his eye that "the method did not appeal to the engineers as highly accurate."

Two important points emerged from his work. One of these, the constant presence of the colon bacillus and closely related forms in sewage—and the absence of these organisms from water known not to have been exposed to sewage contamination—assumes great significance in the light of subsequent work. That he had hopes of finding a reliable biological indicator of pollution is evident from his remarks at the time, ". . . a study of the sewage bacteria as such may throw light on the vexed question of the possible pollution of water supplies; for, if certain species are found to be characteristic of sewage, and are never found in uncontaminated sources, then the presence of these typical 'sewage bacteria' in any given water supply will indicate undoubted pollution We may perhaps look forward to the time when the bacteriologist will be able to say, of a given water: Such and such species of bacteria are present, therefore, at some time sewage must have entered this water; or, on the other hand: Only those species are present which are always found in pure uncontaminated water."

The other point has been generally overlooked by subsequent investigators. Jordan, with Ellen H. Richards, studied the nitrifying bacteria of soil and water. Following the work of the Franklands and others, they reached the conclusion that the microorganisms would not grow on gelatin plates and succeeded

in isolating cultures of nitrifying bacteria in inorganic solutions. Shortly before the experiments were complete, Winogradsky's first report appeared and Jordan's independent work had only the status of confirmation although published in the same year. He and Mrs. Richards were undoubtedly the first in America to study nitrification.

Meanwhile he had spent a summer or two at Woods Hole and there met Charles Otis Whitman and some of the other zoologists with whom he was to be associated later. As a result of Whitman's influence, Jordan became interested in zoology and experimental embryology and through him obtained a two year fellowship at the newly founded Clark University in Worcester, Massachusetts. He went to Clark in 1890 to finish his formal training with Whitman and received his Ph.D. in 1892. The new environment was highly congenial in many ways. Clark University had been founded by G. Stanley Hall for the purpose of establishing a strictly graduate and research institution. Whitman's own mind had much in common with this ideal. Speaking of the graduate student, he had said, "He is recognized, not as an irresponsible school-boy, to be marked for absences, ranked for recitations, and rewarded, after a prescribed number of years of study and decent behavior, with a 'graduating degree'; but as a man who knows, or ought to know, his purpose, and who, if he ever expects to attain the distinction of a degree, must demonstrate his eligibility thereto by making some worthy contribution to the advancement of knowledge in his chosen field." This ideal, together with Whitman's strong conviction that a young man had best be given a problem and left largely to his own devices in working it out, were in complete accord with Jordan's own attitude. Despite the discrepancy in their ages—Jordan was twenty-four and Whitman fifty-eight—Jordan developed a great admiration for the older man and was undoubtedly greatly influenced by him. Both were interested in fundamental rather than superficial biological phenomena and shared the conviction that devotion to research was a prime means and chief end of higher education. Jordan developed a warm friendship with the other younger men, Shosaburo Watasé, Frank R. Lillie and William Morton Wheeler, and his associa-

tion with them went far in adding to his enthusiasm for his scientific work and in keeping his mind out of what might readily have become a narrow rut of sanitary biology. He was engaged at the time to Elsie Fay Pratt, whom he married shortly afterwards.

The experimental work he did at Clark University was reported in two papers. One of these was a study of the spermatophores of Diemyctylus and the second, entitled "The Habits and Development of the Newt," was his doctor's thesis. In the latter piece of work he supplemented the usual histological methods of study by continuous observations on the living embryo during gastrulation, and was able to see the movement of surface cells over the lips of the blastopore. This and other observations led him to support the theory of invagination rather than that of delamination in the formation of the mesoderm—a conclusion which has been strongly supported by later work.

When Whitman was offered the chair in zoology at the newly organized University of Chicago, Jordan readily accepted the invitation to accompany him and the other men in the department to Chicago as an instructor in zoology. The University opened in the fall of 1892 and Jordan began the active part he played in the development of the institution with which he was to remain the rest of his life. He was twenty-six years old when he went to Chicago.

Jordan's interest in the rapidly developing science of bacteriology and the application of the new knowledge and its methods to preventive medicine soon became evident in concrete form. In the spring of 1893, the first year he was at the University, he gave a course entitled "Sanitary Biology" which the announcement for that year describes as "The sanitary problem. The methods, objects, and results of the examination of drinking water; the examination of air, soil, milk, ice, etc. Sewage disposal and water supply. The filtration and precipitation of sewage. The nitrification of organic matter. Lectures and seminar." Wheeler gave the courses in embryology, Watasé those in cytology and Lillie finished his graduate work.

The following year, 1893-'94, Jordan expanded the course in sanitary biology to two courses, one in general bacteriology and

the other in advanced bacteriology. He gave, in addition, a course in general biology. He had already started experimental work in bacteriology, a study of the typhoid bacillus and the methods of differentiating and identifying it. He had not altogether abandoned experimental zoology at the time, for in 1894 he published with Eycleschymer a study of the cleavage of amphibian ova. After this, however, he devoted himself to bacteriology entirely insofar as his own experimental work was concerned and published no more work in pure zoology.

The next year he was made an assistant professor. He gave the same courses and continued his experimental work with the typhoid bacillus, a study of the conditions affecting its behavior in water. He went to Europe for a short time in the spring of 1895 and spent the six weeks he had available there at the Pasteur Institute in Paris. Although there was no time for extended experimental work, the stimulation and the contacts with men such as Roux and Duclaux were valuable and he took full advantage of the opportunity to obtain first hand information with regard to European practice. He returned to Chicago for the year 1895-'96, gave again his courses in general biology and elementary zoology but expanded the work in general bacteriology to two courses, one introductory and dealing with the biological and sanitary aspects of bacteriology and the other covering the pathogenic bacteria, disinfection, *et cetera*.

By 1897-'98 bacteriology at the University of Chicago began a rapid expansion under Jordan's direction. Charles Manning Child joined the faculty and took over the course in elementary zoology, leaving Jordan more time to devote to bacteriology. Albert Lincoln Smith, who had received a Ph. D. from the University of Berlin seven years before, registered as a special student in the University and gave a course in water and water supplies. Howell Emlyn Davies, a graduate student who held a fellowship in bacteriology, gave a course in elementary bacteriological technic. Jordan continued with general and pathogenic bacteriology and, in addition, introduced a seminar in immunity. The lack of texts of bacteriology in English for the benefit of those who did not read German with facility had been evident for some time, and Jordan attempted to fill in the

gap with a translation of Ferdinand Hueppe's well-known "Naturwissenschaftliche Einfuhrung in die Bakteriologie" into English under the title of "The Principles of Bacteriology." The translation was published in 1899 and although regarded as a remarkably good translation of difficult, idiomatic German, did not enjoy a great popularity. Its lack of popularity undoubtedly had roots in Hueppe's highly individualistic concept of disease. He differed from Koch in that he felt that disease was a process resulting from varying causes and that on dynamic principles the most important cause was to be sought in the structural idiosyncrasies of the patient rather than in the invading bacteria which he regarded, at the most, as "liberating causes."

Jordan had, by this time, become thoroughly convinced of the importance of bacteriology as a separate science rather than a branch of some other biological science, and felt strongly that bacteriologists should have a society of their own. He was not alone in this, of course, but he and H. W. Conn of Wesleyan University were the only ones who campaigned actively for the organization of such a society. Their efforts were so successful that the first meeting of the Society of American Bacteriologists was held in New Haven in 1899 with a program arranged by A. C. Abbott. Jordan was very active in the Society for a number of years, holding the office of president in 1905, but in later years his active participation declined. His part in the organization of the Society was, perhaps, as typical of the man as any other single project. He felt that such a Society was actively needed and he spared no effort and inconvenience to get it under way. He actively supported it in its early struggling days, but when it became clear that the Society had developed into a healthy, strong organization, he characteristically left it to be conducted by the younger men, but always retained a warm personal interest in it.

Meanwhile the work at Chicago was developing rapidly. Jordan was made associate professor in 1899, the last year bacteriology stayed in the Department of Zoology. Davenport had become a member of the staff and took over the course in general biology so Jordan's teaching time could be devoted entirely

to bacteriology. He made use of this additional time by offering a course in public hygiene and found it necessary to have Davies help him with the general course. Smith again gave the course on water and water supplies. Jordan's own experimental work continued with an extensive study of *Bacillus pyocyaneus* and its pigments and another study of the production of fluorescent pigments by bacteria. His broad, general biological view of bacteriology is evident not only in his papers on bacteria, but even more in the variety of aspects of bacteriology and preventive medicine which interested him. In 1898 he read a paper before the Chicago Medical Examiners Association on the supposed inheritance of bacterial diseases and in 1899 published a study of the death rate from diphtheria in the large cities of the United States. It was clear to him from the beginning that preventive medicine and hygiene depended not only on purely bacteriological and immunological studies but also on the general biological aspects of host and parasite populations, the mechanisms involved in the transmission of infective agents and many other factors.

In 1900 bacteriological work was removed from the Department of Zoology and incorporated in a new Department of Pathology and Bacteriology, of which Ludvig Hektoen was in charge. The change was with respect to administration, for the laboratories continued to be housed in the Hull Biological Laboratories. Bacteriological work had expanded to such an extent that the entire fourth floor of the Hull Laboratories was occupied. The change, of course, included a change in the staff with whom Jordan was associated. Smith and Davies no longer worked in the bacteriological laboratories and Jordan's associates in the new department included H. Gideon Wells and William Buchanan Wherry in addition to Hektoen. Wherry was an assistant in bacteriology. Very much the same work in bacteriology was offered—courses in general and pathogenic bacteriology, elementary technic, public hygiene and research. Ernest E. Irons, Howard Taylor Ricketts and Mary Hefferan came to the department the following year, and Wilfred Hamilton Manwaring in 1903.

During these years a situation developed in Chicago which was, in a sense, made to order for Jordan with his training and experience. For many years the city of Chicago had been discharging its sewage into the Chicago River and, as the population increased and the volume of sewage grew, the water intakes were moved farther and farther out into the lake to escape sewage contamination. By the late eighties, the situation had become critical. Typhoid fever was common in the city, a serious epidemic occurred in 1890-'91, and minor epidemics were frequent. In a report of an investigation of the quality of public school water supplies conducted by Jordan in collaboration with F. R. Zeit and J. H. Long of Northwestern University, he wrote ". . . Through most of the time during the period covered by our examinations the water has not been in a safe sanitary condition. . . . It should be added that its condition now is probably not worse than has been the case many times in the last fifteen years. . . . The real source of the difficulty is in the quality of the general public water supply. . . ." The contaminated water supply assumed somewhat greater importance to the city at this time because of the approaching World's Fair It was so bad that the *Lancet* went to the length of appointing a commission to study the sanitary aspects of Chicago city water with the object of providing unbiased information for Englishmen who might come to the Exposition. Needless to say, the report of the commission was not flattering. Meanwhile the city had taken action and had organized the Sanitary District of Chicago under a general law enacted by the State Legislature in 1889. The remedy applied through the agency of the District was the cutting of the Drainage Canal from Bridgeport to Lockport and thereby reversing the flow of the Chicago river. Sewage was, therefore, no longer discharged into the Lake but drained into the Mississippi River by way of the Illinois and Desplaines Rivers. The remedy was efficacious as far as Chicago's water supply was concerned but resulted in a suit between St. Louis and Chicago in which the former charged that the sewage which eventually found its way into the Mississippi River resulted in contamination of the St. Louis water supply.

Jordan was intimately associated with this sewage disposal scheme from its beginning and carried on extensive bacteriological examinations of the water of the Illinois River at various points both before and after the Canal was opened. His investigations were the first controlled and extensive examinations of the question of self-purification of streams and provided a solid foundation of fact upon which all later work has been based. Briefly, he found that the enormous numbers of colon bacilli present in Chicago sewage disappeared completely in less than one hundred and fifty miles of flow and that the bacterial flora of the Illinois River at Grafton, where it empties into the Mississippi, was not altered by the opening of the Drainage Canal. His testimony on this point before the Supreme Court of the United States was the decisive factor in its decision in favor of the city of Chicago. Jordan's keen mind was nowhere displayed to better advantage than on the witness stand before the Court. Aside from the legal implications of his work, he had established the fact of self-purification of streams, a tremendously important addition to knowledge of sanitation and one upon which innumerable sewage disposal systems have since been based. It was not alone the scientific interests of the matter that motivated his work. He remarked on another occasion that "It is one of the vital offices of a university to contribute to the well-being of the community in which it is placed." His contributions to the sewage disposal of the city were, in effect, a partial discharge of the obligation he felt, and he continued to discharge this obligation further in following years through his association with the public health activities of the city. The present high standard of public health in Chicago is due, in large part, to his influence and advice.

Since the translation of Hueppe's text of bacteriology had failed to satisfy the need for an adequate text in English, Jordan filled the gap by writing one of his own. The idea had undoubtedly been germinating for some time, and in 1902 he entered into an agreement with the W. B. Saunders Company for the publication of a text which he would write. His book, "General Bacteriology," was a carefully considered and carefully written volume, one which he took six years to write—the

first edition appeared in 1908. His facility for writing beautiful English stood him in good stead and the immediate and continued success of his text is no doubt attributable to its well written, orderly and accurate presentation, as well as to the need it filled. For many years Jordan's "General Bacteriology" was, by all odds, the most widely used text in this country and had gone through eleven editions at the time of his death. Through the agency of this volume, Jordan may be said to have exerted a strong and wide influence on the development of American bacteriology and it was one of his important contributions to the field.

The John McCormick Institute for Infectious Diseases had been founded in 1902 by Mr. and Mrs. Harold F. McCormick. Ludvig Hektoen was director of the Institute and in 1905 Jordan was placed in charge of the Serum Division. At the time antitoxic sera were prohibitively high in price and altogether beyond the reach of the general public. The situation appeared unreasonable and Jordan was moved to undertake the production of diphtheria antitoxin under the auspices of the Institute so that such sera might be made available generally. His mind, of course, went beyond the immediate issue, the therapeutic use of antitoxin in the individual case, to the more general public health aspects of diphtheria control. He personally purchased a farm outside the city in Barrington, Illinois, and there the Institute kept horses for the production of antitoxin. Paul Gustav Heinemann, who had come to the department as a fellow in 1904, gradually took over the actual supervision of the work at Barrington, thereby relieving Jordan of much of the detail. Only native serum was produced at the beginning, but in 1907 the Gibson method of refining and concentrating antitoxin was adopted and a few years later the Banzhaf method, one utilizing plasma instead of serum, took the place of the Gibson method. A modification of the latter method was worked out and a more efficient method of bleeding the horses developed. Better serum than that available commercially was produced under Jordan's direction, and the improved methods were published to make them available to others. At first antitoxin was distributed through the Illinois State Board of Health but later, when more

became available, the Serum Division changed its policy so that physicians and druggists could obtain antiserum directly from the Institute or from the City of Chicago Department of Health. The costs were borne by the Institute and the antitoxin was purchased from the Institute, Jordan contributing his time and the use of the farm. It is difficult to estimate the benefit conferred by the serum work as carried out by the Institute. Not only were indigent sufferers from the dread disease relieved but, further, the products of the large manufacturing concerns were materially improved and reduced in cost. The Serum Division was discontinued in 1917 after its purposes had been accomplished.

Jordan was made full professor in 1906. For a period of several years, roughly from 1905 to 1914, his interests went further into the field of public health. His work with water-borne disease took the form of specific investigations of outbreaks of typhoid fever in Milwaukee, Detroit, Des Moines, St. Charles, Winnipeg, Quincy, Rockford and other municipalities. His papers reporting these investigations are models of what an epidemiological investigation should be, and he was invariably able to put his finger on the weak link in the sanitary chain. The detailed bacteriological studies in connection with these epidemics completed the picture of the mechanisms involved in the transmission of the disease. The study of the Rockford epidemic was one of the most interesting of these, for here Jordan found definite evidence that the contamination of the water supply with sewage sometimes resulted in a preliminary gastro-enteritis of relatively short incubation period which preceded the actual infections. As a result of these and other pieces of work, he came to be regarded as one of the foremost authorities in the country on water-borne disease and at the request of the United States Public Health Service, he set up bacteriological standards for drinking water supplied to the public by common carriers in interstate commerce.

He became more and more interested in milk and milk-borne disease, incidentally tracing three outbreaks of typhoid fever to contaminated milk supplies. He was very active in the campaign for pasteurized milk in Chicago. One of his earliest pieces of

work in this connection (1904) was a pointed analysis of the Chicago milk market, carried out while he was a member of the Health and Sanitation Committee of the Civic Federation of Chicago. As a member of the Committee on Regulations for the Pasteurization of Milk, he was, in large measure, responsible for the organization and drafting of a code of uniform and effective practice. Further, he took an active part in creating an informed public opinion on the desirability of pasteurization of milk supplies through articles such as "The Campaign for Pure Milk," published in Christendom, "The Household Milk Supply," a publication of the Domestic Science Department of the University, "The Case for Pasteurization," in the Journal of the American Medical Association, and similar informative writings which reached the medical profession and various portions of the general public. His own enthusiasm was, of course, for the adoption of effective public health measures based upon a solid foundation of scientific fact, and he realized that the hope of achieving such goals lay in the education of the public at large.

In spite of the fact that many of his activities were directed toward specific goals, Jordan never became narrow in his interests. In the midst of studies of typhoid fever epidemics, milk-borne disease and other investigations, he found time for articles such as "The Sphere of Bacteriology," "The School and the Germ Carrier," "Profitable and Fruitless Lines of Endeavor in Public Health Work," "School Diseases," "Disease Carriers Among School Children," and others. In 1912 he began the series of annual studies on typhoid fever in the large cities of the United States which appeared anonymously in the Journal of the American Medical Association.

Meanwhile his experimental work continued along various lines. His interest in insect-borne disease is apparent from two pieces of work on anopheline mosquitoes, one a note on the occurrence and habitat of *A. punctipennis* and *A. maculipennis,* and the other a study on the binomics of *Anopheles.* From the beginning of his work and throughout his life, Jordan was greatly interested in what is often called "pure" bacteriology. He carried on active experimental work of this kind in addition to his public health activities. Work on bacterial

enzymes, on the effect of bile on the colon bacillus, and similar studies may be considered to be in this category. Perhaps one of the most interesting evidences of the breadth of his scientific thinking during this period is a paper on bacterial variation read before the National Academy of Sciences. The strict monomorphism of Koch and his school had, for a time, subdued pleomorphism with the exception of that exhibited by the so-called involution forms. Bacterial variation did not come into its own until the early twenties with the work of de Kruif and Arkwright, and yet in 1914 Jordan perceived the importance of the problem and discussed it at length. His peculiarly analytical mind, which solved complex epidemiological problems in what seemed to many an almost uncanny fashion, is evident through all his work. His early study of a thermostable hemolytic substance present in sterile nutrient broth, an attempt to assay the significance of such variables in studies on bacterial hemolysins, was thoroughly characteristic of the man. Those who worked with him knew of his constant, almost fanatical, demand for control experiments. During this period Jordan became much interested in milk-sickness and carried on extensive experimental work in collaboration with Norman McLeod Harris in an attempt to find a bacterial etiology. These attempts were unsuccessful and it was later shown that the toxic qualities of milk from cows with trembles were a result of poisoning of the animals by white snakeroot.

In 1909-'10 he spent a sabbatical year at Freiburg. During the year he did little or no experimental work but spent his time studying the sanitary organization of the larger German cities, such as Frankfort, Berlin and others. Germany was, of course, much further advanced than the United States in this respect, and Jordan was particularly interested in the methods used in training sanitarians and the organization of instruction in hygiene. The information he gained was put to good use when he returned to America.

For some time Jordan had felt that the scope of bacteriology was so broad that its possibilities, particularly with regard to hygiene and preventive medicine, could not be realized in a Department of Pathology and Bacteriology. The rapidly grow-

ing importance of preventive medicine convinced him that instruction in this field should be made available in medical schools, and the medical school at the University in particular, and further that an opportunity should be provided for the training of health officers and experts in the field. His efforts and enthusiasm bore fruit in the creation, in 1912, of the new Department of Hygiene and Bacteriology with Jordan as head of the Department. The change was, again, one of administration, for the laboratories continued to occupy the fourth floor of the Hull Biological Laboratories. The first year the staff included, in addition to Jordan, Harris who had been in the old department since 1902, Heinemann, and Wherry, who had left but returned as a visiting professor. Wherry gave a course in parasitology, the first in the University, and Jordan expanded his own teaching to include a course on vital statistics and epidemiology. The introduction of parasitology into the Department was an innovation in the field of bacteriology. The protozoan and helminth infections and the role of insects in the transmission of disease had, of course, been known for many years, but Jordan was one of the few who perceived the essential similarity and common ground between the two fields. In later years it has been more generally realized that parasitic and bacterial infections have much in common, not only in modes of transmission, but also in the defensive mechanisms of the host against the infective organism which, in many cases, appear to be identical. Jordan hoped, from these beginnings, to develop a school of hygiene and public health that would function side by side with investigative work of a fundamental nature. This ambition was never quite realized, owing to a variety of circumstances, but the plan served as a stimulus for the development of such work in American universities.

By 1915 the Howard Taylor Ricketts Laboratory had been built by the University to serve as temporary quarters for the Departments of Pathology and of Hygiene and Bacteriology, and after twenty-three years bacteriological work at the University was physically separated from the Department of Zoology, where it had originated. The new laboratory was named in honor of Ricketts, a member of the faculty of the Department

of Pathology and Bacteriology who had died in 1910 of typhus fever during an investigation of that disease in Mexico.

During these years Jordan's interests had become even broader and included the field of food poisoning. The transition from the water- and milk-borne enteric diseases to gastro-enteritis resulting from the ingestion of foods was not a difficult one. The expansion of his interests in this direction was undoubtedly facilitated by his contacts with the large meat packing concerns in Chicago. He had been asked by one of these companies to investigate a stubborn outbreak of typhoid fever in a subsidiary plant in South America. He solved the problem in his usual competent fashion and at the same time developed a marked interest in the problems of food preservation and food poisoning which confronted the packing industry. This interest soon took concrete form in the shape of several publications in the general field of the bacteriology of foods, food-borne infections, and food-poisoning. He began at this time his association with the packing industry as an advisor and consultant, an association which lasted until his death. The step took courage on his part for, at the time, it was generally felt that industrial connections were not altogether desirable for one engaged in academic work. The opportunity to contribute further, although indirectly, to public health was not to be denied and the sanitary improvements resulting from close cooperation between industry and the research laboratory have amply sustained his feeling.

About this time Jordan started an extended investigation into the differentiation and biological characteristics of bacteria of the typhoid-paratyphoid group. The morphological, physiological and immunological similarities of these organisms made differentiation of the species from one another a difficult matter and a possible solution lay in a detailed and careful study of the entire group. The results of this work were embodied in a series of papers extending over a period of years, the last paper appearing in 1925. As a result of this investigation and a number of others, he became the foremost authority on these organisms in America.

When the United States entered the World War in 1917 two sanitary needs were apparent at once; a supply of trained tech-

nicians for laboratory diagnosis, and organization and supervision of the laboratories at the training camps in this country. The problem which arose was the control, not of enteric infections as in previous wars, but of respiratory infections, such as pneumonia and epidemic influenza and of meningococcus meningitis. The last was of greatest importance in the early days, the influenza control came later. Jordan, in common with the other bacteriologists of the country, rose to the occasion and bent every effort toward the control of these diseases in the army camps. His work took two forms; one, the most obvious, was the training of technicians for work in the army laboratories. The University laboratories were used to a great extent for this purpose, the courses of training being arranged and taught under Jordan's direction. The other work he undertook was in the capacity of director of the Red Cross car "Lister." Four of these cars, equipped for field laboratory work, were built by the Pullman Company and operated by the Sanitary Service of the American Red Cross. Jordan made a series of trips in the "Lister" to army camps in various parts of the country. When the laboratory of a given camp required organization or instruction in diagnostic methods or when an epidemic appeared to be getting out of hand, the car was called and whatever measures necessary were taken. The "Lister" operated only a few months before being turned over to the army along with the "Reed" and "Metchnikoff." One car, the "Pasteur," was retained by the Red Cross and Jordan accepted the directorship of this car after the others had been transferred. The essential weaknesses in the organization for control of disease in the army camps were apparent at once to Jordan, and he made many pointed and valuable suggestions which were perhaps of considerably more importance than the actual work of laboratory organization and instruction in diagnostic procedures.

The influenza pandemic of 1918-'19 was of great concern, not only to the army but to the civilian population as well. The problems confronting the bacteriologists were difficult ones, particularly since Pfeiffer's bacillus, thought to have been established as the causative agent of influenza many years before, was found to bear only an uncertain relation to the disease. As

a member of the Respiratory Commission, Jordan undertook a systematic investigation of the bacteriology of the disease, an investigation in which not only he but other members of his department took an active part and which extended over a period of several years. Other members of the Commission made similar investigations and by frequent consultation and pooling of information it was hoped that some light might be thrown on the etiology of the disease. The venture was not successful, however, and in 1927 Jordan wrote an extended review entitled "Epidemic Influenza" which was published in book form by the American Medical Association and which did much to clarify the chaotic mass of information which had accumulated about the disease in the course of years. His serious consideration of certain experimental evidence suggesting a virus etiology is of particular interest in view of the recent work in which a filterable virus has been shown to be the cause of at least some kinds of influenza.

After the brief disorganization of the war period, the department settled down to continued development under Jordan's guidance. An additional laboratory, Ricketts Laboratory South, had been built and a few years later, when the Department of Pathology moved to new quarters in Billings Hospital, the Department of Hygiene and Bacteriology occupied both buildings. The work in parasitology, begun with Wherry in 1913, took more definite form with the addition of William Hay Taliaferro to the staff in 1924. Taliaferro initiated and maintained active research in the field which in its fundamentals drew closer to bacteriology.

Not long afterwards, research on virus diseases was initiated in the department—at first confined to poliomyelitis but later, with the addition of N. Paul Hudson to the staff, extended to include a variety of studies on other diseases of virus etiology.

Jordan's own work on the paratyphoid-enteritidis group and his epidemiological work went on unchanged. He undertook an annual report on diphtheria mortality in the large cities in the United States, a companion to the annual typhoid report, which was likewise published anonymously in the Journal of the American Medical Association. He prepared standard methods of

bacteriological analysis of milk for the American Public Health Association and published papers on the bacterial content of stored normal and typhoid feces and the interconvertibility of "rough" and "smooth" bacterial types. His experimental work turned to studies on food poisoning and food-borne infection and the relation of the paratyphoid bacilli to these problems. He had, by this time, become accepted as the first American authority on food poisoning and shared international honors only with Savage of England. The food poisoning investigations took a new and promising turn with Dack's discovery of a filterable substance produced by staphylococci which, on feeding to human beings, produced the typical clinical picture of food poisoning. The importance of this observation was obvious to Jordan and the phenomenon was subjected to an intensive investigation. Jordan's observation that certain monkeys were susceptible to the action of the toxic substance was soon confirmed by the other workers in the laboratory and put the study of the toxic substance on a solid experimental foundation. He later found that a variety of bacteria, including the presumably innocuous colon bacillus, could produce similar toxic material under suitable conditions of cultivation. This finding was of some interest in that it was in complete accord with epidemiological studies. Savage had previously postulated the existence of such toxic substances on theoretical grounds, an hypothesis with which Jordan did not agree until their existence could be shown experimentally. The earlier portions of this work were summarized, in a general review of food poisoning in book form entitled "Food Poisoning and Food-Borne Infection," published in 1931. The volume was, in effect, a second edition of an earlier book, "Food Poisoning," published in 1917. Characteristically, in neither of these did Jordan regard food poisoning as a purely bacteriological problem but presented, in addition, a complete and concise summary of food poisoning resulting from contamination of food with toxic chemical substances.

In the last few years of his life, Jordan took his vacations in the winter and spent them in Puerto Rico, Panama and Jamaica. At no time did he stop working, for in Puerto Rico he was visiting professor in the School of Tropical Medicine, in the

Canal Zone he worked in the Gorgas Memorial Laboratories—it was here that he discovered the susceptibility of monkeys to the staphylococcus toxic substance. In Jamaica he became interested in an affection peculiar to that island and certain other parts of the world which was called "vomiting sickness," and by a judicious combination of epidemiological and experimental work, disclosed significant facts relating to its etiology. While at these places he gathered information regarding the general sanitary situation which he presented to the Rockefeller Foundation in the form of informal reports—he had been a member of the International Health Division for some years and then a member of the Board of Scientific Directors of the Foundation.

Jordan was appointed Andrew McLeish Distinguished Service Professor of Bacteriology in 1931 and was retired in 1933 at the age of 67. He continued to work as actively as ever after retirement but was not well during the last year of his life, his illness the result of a coronary occlusion. During the latter part of the summer of 1936 he and Mrs. Jordan went to Shelburne, Vermont, for rest and recuperation in the New England that was always dear to him. Here his condition became suddenly worse and he was removed to the hospital in Lewiston, Maine, where he died.

He had lived a full and active life and the honors that had come his way were many. He had been president of a number of organizations, including the Society of American Bacteriologists, the Epidemiological Society, the Chicago Pathological Society and the Institute of Medicine. He served as a member of the Board of Scientific Directors of the International Health Division of the Rockefeller Foundation, on the Medical Fellowship Board of the National Research Council, as a trustee of the McCormick Institute, a member of the Committee on Foods of the American Medical Association, as a consultant to the United States Public Health Service, as consulting bacteriologist to the Stream Pollution Laboratories of the Service and in many other capacities. A Fellow of the American Public Health Association, he was the recipient of its Sedgwick Medal in 1934. He was on the editorial boards of a number of scientific journals, was editor of the Journal of Preventive Medicine and joint

editor, with Hektoen, of the Journal of Infectious Diseases. He gave several endowed lectures, including the Gordon Bell Memorial Lectures, the Harvey Lectures and the Cutter Lectures. He was made an honorary Doctor of Science by the University of Cincinnati in 1921 and was elected a member of the National Academy of Sciences in 1936.

His students made up a very important part of his life. He knew no greater joy than that of developing to the best of his abilities the talents of promising young men and women and he followed their subsequent careers with keen personal interest. They, in turn, felt his warm and steady support and did not hesitate to call upon him for advice and encouragement in later years. He is said to have remarked upon one occasion that if he had done no productive research, he would still feel that his students alone would have made life well worth living.

Jordan's contributions to bacteriology, public health and preventive medicine in America can hardly be over-estimated. The leading American authority in the fields of his greatest interest, his influence was great and many of his contributions were of the subtle kind that escape general notice. His scientific acumen and the uncompromising probity with which he dealt with the problems he handled left a permanent impress on bacteriological thinking.

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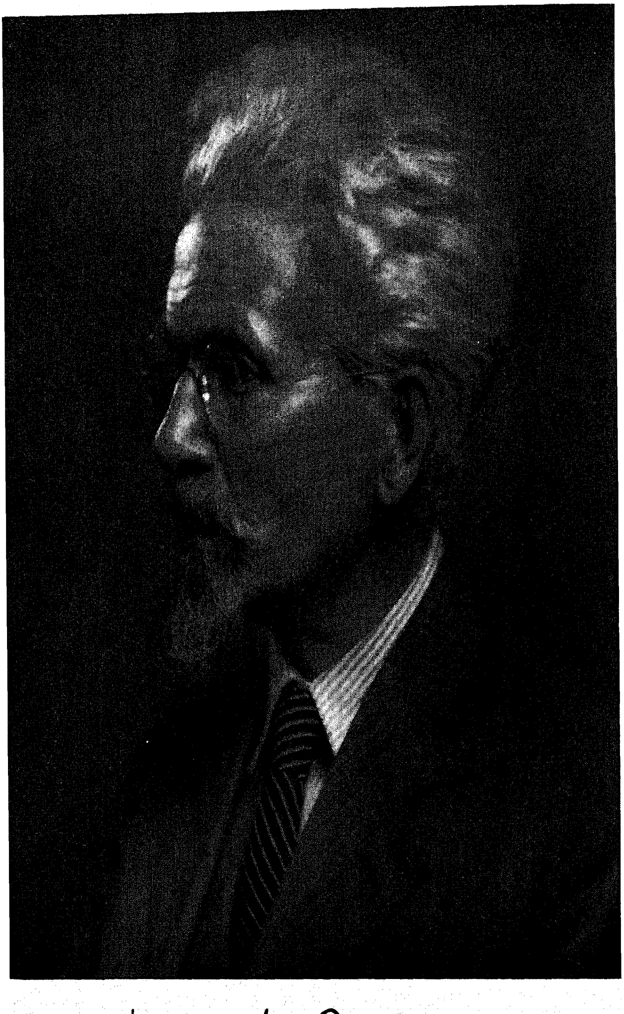

Henry H. Donaldson

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OF

HENRY HERBERT DONALDSON

1857–1938

BY

EDWIN G. CONKLIN

PRESENTED TO THE ACADEMY AT THE AUTUMN MEETING, 1938

HENRY HERBERT DONALDSON

1857–1938

BY EDWIN G. CONKLIN

Henry Herbert Donaldson was the son of John Joseph and Louisa Goddard (McGowan) Donaldson. Both his parents were of Irish stock though both were born on this side of the Atlantic; his father was a native of New York where he was a successful banker; his mother was born in Montreal, Canada. She was a handsome woman, noted for her orderliness and great presence of mind, in which qualities her son resembled her. Both his parents lived to an advanced age, his father dying at 79 and his mother at 84, his grandparents also were equally long lived. Their children were Henry Herbert, born at Yonkers, New York, May 12, 1857, and Alfred Lee, born in 1866. The latter like his father was a banker; he was also the author of a "History of the Adirondacks" and was something of a poet and musician. The elder son prepared for college at Phillips Academy, Andover, Massachusetts; he then entered Yale and was graduated in 1879 with the degree of Bachelor of Arts. His father had desired that his son should join him in business but yielding to the young man's preference for science it was agreed that he should enter the medical profession. In further preparation for medicine he spent an additional year at Yale working with Professor Russell H. Chittenden on the detection of arsenic in various organs of the body in cases of arsenical poisoning. The results of this year's work were published jointly with his professor as his first scientific paper. (See appended bibliography.)

During the year 1880–81 he attended the College of Physicians and Surgeons in New York but realizing that his interests were in research rather than medicine he accepted a fellowship in biology in the graduate school of Johns Hopkins University in 1881, which appointment he held for two years, specializing in physiology under Professor H. Newell Martin. As a result of this work he published four papers on physiology and pharmacology, two of them under joint authorship with other students. (See bibliography.)

After his two years as fellow he was appointed student assistant in the department of biology for the year 1883–84. At the close of this academic year he married Julia Desboro Vaux of New York and he and his bride spent the summer of 1884 at Beaufort, North Carolina, in Professor Brooks' laboratory. During the following year he finished his thesis for the doctor's degree under the direction of G. Stanley Hall who was at that time professor of psychology at Johns Hopkins, and was awarded the Ph.D. in 1885. His thesis was entitled "On the temperature sense" and concerned the mapping of heat-sensitive and cold-sensitive areas of the skin. During the following year he was Professor Hall's assistant and they completed and published jointly a paper on "Motor sensations of the skin".

His work on the sensory areas of the skin led him to seek more extensive training in neurology in European centers, and in February 1886 he and his wife and his father went abroad and remained abroad until the autumn of 1887. During their year and a half abroad he worked in Forel's laboratory at Zürich and von Gudden's at Munich and spent some time with Meynert at Vienna and Golgi at Pavia. At the close of this European visit he returned to Baltimore as associate in psychology at the Johns Hopkins University and this position he held from 1887 to 1889.

During his years at Johns Hopkins he was associated or acquainted with a group of unusually able and stimulating fellow students in biology. William T. Sedgwick and Edmund B. Wilson had received the Ph.D. degree the year he entered but K. Mitsukuri, H. W. Conn, Frederick S. Lee and J. Playfair McMurrich were fellow students. In his second year as fellow (1882–83) J. McKeen Cattell and William H. Howell were also fellows in biology, and the following year Adam Bruce and Ethan A. Andrews were fellows in biology, while fellows in other departments included Henry Crew, John Dewey, Woodrow Wilson and other men of brilliant mind who later attained wide distinction. This stimulating fellowship undoubtedly had a great influence on young Donaldson.

In 1888 G. Stanley Hall became president and professor of psychology in the newly founded Clark University at Worcester,

Mass., and in 1889 Doctor Donaldson was called to Clark as assistant professor of neurology. His proven ability in this field led President Hall to assign to him for study the brain of Laura Bridgman, a blind deaf mute who had been taught to speak and had attained marked mental ability. Parts of this long and detailed study were published in 1891 and 1892 and it has been characterized as "probably the most thorough study of a single human brain that has ever been carried out". Of this study Dr. Donaldson has written in a brief autobiographical note: "The chief modifications found in this brain were caused by an arrest of growth due to the destruction of the sense organs. This made it desirable to know the developmental state of the brain at the time of the destructive illness (two years). Such information was not in the literature. With the hope of contributing to fill this gap I arranged a program for the study of the brain (nervous system) from birth to maturity. In carrying out this plan quantitative methods were used and data on the size and weight of the parts and on the number of cells in them were especially considered."

This led him to that long, accurate, quantitative study of growth which was the main theme of his life work. He gathered together all the available material on the growth of the central nervous system and published it in a book entitled, "The growth of the brain: a study of the nervous system in relation to education" (Scribner, 1895).

He remained at Clark University until 1892 when he joined the migration of many of the faculty to the reorganized University of Chicago, where he became professor of neurology and was very active in the development of the scientific departments. From 1892 to 1898 he served as dean of the Ogden Graduate School of Science of the University of Chicago. During this time a tubercular infection of the hip joint interrupted his work and left him permanently lame, but after a prolonged visit to Colorado he returned to his work with courage and determination and from 1898 until his death there was never a year when he did not publish one or more researches.

In 1905 ten professors of anatomy and zoology in leading universities were invited to serve as the Scientific Advisory Board of the Wistar Institute of Anatomy and Biology in Philadelphia. Among these were Charles S. Minot of Harvard, George S. Huntington of Columbia, Franklin P. Mall of Johns Hopkins, George A. Piersol and Edwin G. Conklin of the University of Pennsylvania, Simon H. Gage of Cornell, G. Carl Huber and J. Playfair McMurrich (later at Toronto) of the University of Michigan, Lewellys F. Barker of Chicago (later at Johns Hopkins) and Henry H. Donaldson of Chicago.

On the invitation of the founder of the Institute, General Isaac J. Wistar, and of its Director, Dr. Milton J. Greenman, this Advisory Board met at the Institute in April, 1905 and was asked to propose a plan for the future development of the Institute. It was the unanimous opinion of the Board that the Institute should be devoted primarily to research and in the beginning to research in neurology. This met with the hearty approval of General Wistar, and the Board was asked to recommend some one to organize this work. Dr. Donaldson was the unanimous choice of all the other members and after long and serious consideration he accepted the appointment, and in the following year transferred his activities and his chief assistant, Dr. S. Hatai, from the University of Chicago to the Wistar Institute where he became professor of neurology and director of research.

While at Chicago Donaldson had published one book and seventeen papers, most of them on the human nervous system and that of the frog. However, his attention was called to the peculiar advantages of the albino rat as a laboratory animal in 1893 when Dr. Adolph Meyer had used the rat in a course on the anatomy of the nervous system. One of Dr. Donaldson's associates, Dr. Hatai, had published fifteen papers based on the white rat before removing to Philadelphia. This work on the rat convinced Donaldson that it was the best available mammal for laboratory work on problems of growth. In an autobiographical note he says: "I selected the albino rat as the animal with which to work. It was found that the nervous system of the rat grows in the same manner as that of

man—only some thirty times as fast. Further, the rat of three years may be regarded as equivalent in age to a man of ninety years, and this equivalence holds through all portions of the span of life, from birth to maturity. By the use of the equivalent ages observations on the nervous system of the rat can be transferred to man and tested. The results so obtained show a satisfactory agreement and indicate that the rat may be used for further studies in this field."

For accurate quantitative studies of growth it was necessary to establish a standard stock and to get rid as far as possible of individual differences caused by peculiarities of heredity or environment. Accordingly he and his associates set about the problem of producing a pure bred stock raised under accurately controlled conditions which would give a standardized strain of laboratory animals. How well they succeeded in this is recognized throughout the world by the wide use in the most accurate work of the "Wistar Institute stock" of white rats.

This work on the standardization of a research mammal was summarized in 1915 in a book entitled, "The Rat: Reference tables and data for the albino rat and the Norway rat" (Memoir of the Wistar Institute). A revised and enlarged edition of this book was published in 1924. It may be said that this work renders a service in mammalian research comparable to that of pure chemicals in chemical research or to the "Tables of Physical Constants" in the physical sciences. Largely because of this work the albino rat has become the most widely used laboratory mammal.

The major theme running through the whole of Donaldson's work was organic growth. In his presidential address before the Association of American Anatomists in 1916-17 he said: "Were I asked to name some direction in which we might extend our work I should naturally lay weight upon post-natal growth in the terms of cell multiplication and cell structure, with its many subsidiary problems." Much of this work was on the nervous system, but it was extended to include muscles, viscera, skeleton and teeth, both in normal and in experimental conditions. Several papers were on the number of nerve cells and fibers, others on the size of these, and many papers were

devoted to the effects of domestication, exercise, feeding, castration and age on the size and weight of particular organs. The determinations of the percentage of water in the central nervous system under all of the experimental conditions named constitute a large section of his work.

Altogether he personally published nearly one hundred papers and monographs on these subjects, and his students, assistants and associates published more than three hundred and sixty separate articles on these and related subjects. His method of directing the research work of associates is well described in his published report to the Wistar Advisory Board in 1925 (pp. 46-48): "No investigator is ever asked to do anything which is not for his individual and scientific welfare. For the most part those who come to the Institute are in the early stages of their scientific work and do not bring their problems with them. It is for us, therefore, to suggest one. . . . As an aid in obtaining orderly data which will interlock, the custom of dealing with papers used by the distinguished physiologist Carl Ludwig has been followed. The papers of the younger men have been in every case read critically by some member of the staff familiar with the field, with the new observations brought into relation with those previously published from the Institute. Such criticism assists the younger writer in several ways and also makes it possible to tie together the results of consecutive studies in a manner that gives them cumulative value.—In every case the investigators receive full personal credit for their work. This is as it should be, for it is the virture of academic laboratories that the emphasis is put on the individual rather than on the institution."

Among the many fellow workers who were associated with him at the University of Chicago and the Wistar Institute the following should receive especial mention: Irving Hardesty, S. Hatai, Alice Hamilton, John B. Watson, S. W. Ranson, W. H. F. Addison, Ezra Allen, A. W. Angulo, G. E. Coghill, Eunice C. Greene, Frederick S. Hammett, Helen Dean King, W. and M. C. Koch. In addition to these some forty other investigators were associated with him in his work.

His interest in and sympathy with all types of good scien-

tific work were broad and generous. He had keen appreciation of good literature, music and the graphic arts. His concern with social problems and human welfare was deep and genuine, and even his closest friends never learned from him of his generous contributions and acts of kindness to those who were in need. His students and colleagues knew him as a man of infinite patience, even temper and nobility of character and they loved and honored him. Among those who published work done in his laboratory were some thirty Americans, twenty Japanese and a smaller number of other nationalities; many of these persons are leaders in their professions and all of them revere his memory.

For his eminence in research he received the honorary degree of Sc. D. from Yale in 1906 and from Clark University in 1937. He was elected president of the Association of American Anatomists for 1916-18, of the American Society of Naturalists in 1927 and of the American Neurological Association in 1937. He was elected to membership in the American Philosophical Society in 1906 and was a Councillor of the Society for four terms of three years each, namely, 1911-13, 1915-17, 1928-31, 1932-36; he was chairman of the Publication Committee from 1932 until his death and was instrumental in establishing the new series of *Memoirs* of the Society, and from 1935 until his death he was a vice-president of the Society. He was a member of the Corporation of the Marine Biological Laboratory from its foundation in 1888 until his death and a Trustee from 1912 to 1929 when he became Trustee Emeritus. In 1911 he established his summer home at Woods Hole, Massachusetts and every summer thereafter, with the exception of two when he was abroad, he carried on his research work there at the Marine Biological Laboratory. He was elected to membership in the National Academy of Sciences in 1914 and was a member of the Council in 1919. He was also an honored member of ten other American and foreign scientific societies. For twenty years he was president of the Lenape Club of the University of Pennsylvania and on the occasion of his eightieth birthday in 1937 a bronze portrait medallion of him, made by Dr. R. Tait McKenzie, was placed in the Club

with appropriate ceremonies and a replica of the medallion now hangs in the hall of the American Philosophical Society.

On the seventy-fifth anniversary of his birth, May 12, 1932, a special anniversary volume of the *Journal of Comparative Neurology* was dedicated to him. It was preceded by an admirable portrait and contained a brief sketch of his distinguished career, followed by twenty scientific contributions from associates and friends and the following affectionate testimonial and address:

"Professor Donaldson's long and productive career is an illustrious example of rigid adherence to a well planned program of research on a fundamental theme without wavering or diversion by opportunistic considerations. He is internationally known as a worthy representative of the best traditions of American science and he has won the respect of the scientific world for his consistent and fruitful program of research.

"He has won the esteem and affection of the Editorial Board by unfailing courtesy, loyal friendship and generous support of all worthy enterprises. For his cordial and invaluable cooperation and wise council during nearly thirty-five years the *Journal of Comparative Neurology* owes him much.

"Professor Donaldson: We your colleagues on this anniversary offer our congratulations upon your past achievements, and we rejoice with you in the realization that your productiveness in research and in the wider field of human relationships continues in full vigor and efficiency. We know, too, that the universities and other organizations which you have so ably served in the past and all the numberless friends who have the good fortune to know you personally wish to join with us in this expression of appreciation."

His personal appearance was so distinguished that it always commanded attention and admiration. Any one who had once seen him could never forget his magnificent head, his steady sympathetic eyes, his gentle smile. With these were associated great-hearted kindness, transparent sincerity, genial humor. Perhaps his most distinctive personal characteristic was the quality which Sir William Osler celebrates in his essay, "*Equinimitas.*" With this were naturally associated orderli-

ness, persistance, serenity. His laboratory and library were always in perfect order, his comings and goings were as timely as the clock, he never seemed hurried and yet he worked "Ohne Hast, ohne Rast."

In 1884 he married Julia Desboro Vaux of New York, who died in 1904. Two children were born to them, John C. Donaldson, now professor of anatomy in the Medical School of the University of Pittsburgh and Norman V. Donaldson at present secretary of the Yale University Press. In 1907 he married Emma Brace of New York and their hospitable homes in Philadelphia and Woods Hole were known to a host of loving friends.

After his long illness in the middle nineties of the last century he was never in robust health, but was almost never incapacitated for his regular work. Until a few days before his death he was at work as usual in his laboratory at the Institute. His end came as a result of pneumonia and heart failure at his home in Philadelphia on January 23, 1938, in his eighty-first year. With characteristic foresight he and Mrs. Donaldson had sometime before planned the simple and appropriate funeral service which should be held in the event of either's death. His pallbearers were chosen from among his scientific associates and the officers of the Institute, the University of Pennsylvania and the American Philosophical Society. In accordance with the terms of his will his brain was preserved and added to the notable collection at the Wistar Institute and his body was cremated. His work, influence and memory remain to make the world richer for his having lived in it.

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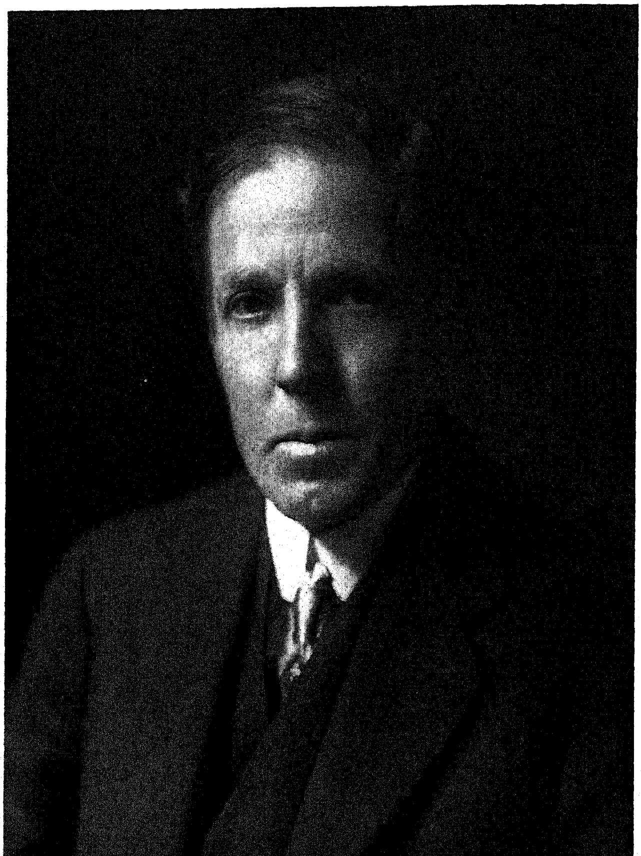

Vernon Kellogg

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OF

VERNON LYMAN KELLOGG

1867–1937

BY

C. E. McCLUNG

PRESENTED TO THE ACADEMY AT THE AUTUMN MEETING, 1938

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Dates, places and events are easy to record and in the life of a man have their significance, but his nature is not revealed thereby. When the time comes to make a record of the real character of one of our friends, we are always oppressed with the inadequacy of our understanding and the feebleness of our expression. David Starr Jordan with two large volumes tried, in "The Days of a Man," to show what it was that made him the man he became. His friend and disciple, Vernon Lyman Kellogg, whose contribution to Jordan's development is so fully recorded therein, leaves for himself no such delineation of character. It remains for us who knew him to do what we can to estimate and record his life and achievements. Such a brief notice as this, however, can serve no further purpose than to anticipate a fuller record which doubtless will be forthcoming.

To the University of Kansas, in the translated New England town of Lawrence, came the young Kellogg in 1885. Here, in this infant institution, he was thrown intimately into contact, as student, assistant and secretary, with scholarly, New England-trained Francis H. Snow, the newly elected Chancellor and an enthusiastic entomologist. They became firm friends and the association proved profitable in every way to Kellogg.

The beginning of his work at Kansas University is characteristic of his whole career. Always the opportunity for the next step—always the ability to utilize it. These opportunities were many and varied and called for a wide range of qualifications—social, scientific, linguistic, humanitarian. But underlying all these qualities and making their application easy and effective was a personality so pleasing, adaptive, persuasive and charming that opposition usually failed to develop. No matter what the type of work that engaged Kellogg's attention, the course of events was much the same. In part this facility of operation resulted from his habit of inconspicuousness. In most of his endeavors he associated himself with some strong and outstanding personality which occupied the public eye and took the

blame or the credit for what resulted. Kellogg meanwhile pursued his studied course, fully aware of the practical bearings of his policies and of the reactions which they awakened. With infinite tact and an uncanny appreciation of personalities, he quietly pursued his way, arousing little opposition and creating no ill feeling. If opposition developed and proved obdurate he did not stress his position, but put the matter aside until a more favorable opportunity. In the event that delay did not improve his chance of success, he made no further effort.

The years at Lawrence were busy ones, but he had time to make many friends—Chancellor Snow, an enthusiast of indefatigable vigor; S. W. Williston, a man of profound understanding and high scientific ideals—an authority in such diverse fields as vertebrate paleontology and dipterology; E. C. Franklin, later a colleague at Stanford, a man of high ideals and achievements; and E. E. Slosson, a writer on scientific subjects of unusual literary ability. The University was young and in the formative stage, with no hindering traditions. Kellogg was free to go his own way and took full advantage of the opportunity. This way led him to activities beyond the campus limits. He wrote for the local paper a column on birds and this led to reportorial work and even to editorial efforts. Thus his urge to write, which later became engrossing, early manifested itself. A desire to travel, which was not so easily satisfied in those days, led to frequent trips to Colorado and elsewhere. Human interest in all his occupations was prominent, whether in efforts to attract people to the study of birds, to protect them from the attacks of injurious insects, or to show them the possible influence of biology in human life. All in all, these years sketched, in broad outline, Kellogg's future course—administration, writing, investigating, interpreting and teaching.

While it is true that the influence of the college years was direct and strong, it is also true that the qualities which they revealed in Kellogg were inherent and had already evidenced themselves in his early youth. William Allen White, who knew him well, in an editorial in the *Emporia Gazette,* gives a charming description of these early days:

"His was a happy boyhood. It was busy and purposeful. It foreshadowed his life. Few boys who have grown up in this town have got so much out of the first years as he did.

"They lived such lives as boys now know only in envious dreams. They skated and swam, trapped and hunted and fished and studied wild life until the whole annual panorama of nature with the going and coming of plants and birds and flowers and the passing colors of the grass and trees became a part of their life.

"Is it a wonder that such a boy became a scientist? How could he help it? When he left this town to go to the University of Kansas in 1885 at 18, his fate was written inexorably in the blood and environment of childhood. A college professor's son, Vernon had learned casually to love the outer manifestations of nature. He yearned secretly to study the inner sources of things."

The few years of experience at the University of Kansas were so fruitful and revealing that they brought Kellogg to the notice of President Jordan, and without hesitation, he offered Kellogg a position on the faculty of Stanford University. Here he came intimately into contact with President Jordan, just as he had with Chancellor Snow at Kansas, and with him he collaborated in teaching and writing. The association was stimulating and helpful in many ways both to Kellogg and to Jordan; fruitful to the University—and to its students.

Here the greatest amount of Kellogg's scientific writing was done, and here he practically ended his career as a teacher and investigator. In view of his great local influence it is curious to note how few were his contacts with fellow biologists in their organizations, and how slight the recognition of his excellent work. This is probably due to the fact that he placed emphasis upon the popularization of biology rather than upon its extension. And yet his scientific bibliography alone would do great credit to any investigator. His sustained interest over many years in the Mallophaga made him the leading authority in this group. But this taxonomic work was only incidental to the question of the evolutionary importance of the biting lice. This same phylogenetic interest he carried over into the study of other insect groups. There was, in his mind, always the broad significance of the biological facts he had discovered. Even the extensive experimental work on the silk worm, extending over a

period of fifteen years, traced back to this interest. The character and extent of his writings are so well revealed in his bibliography that they need not be mentioned further.

The years at Stanford stand out as those of his greatest scientific productivity. A constant stream of books, reviews, addresses and research papers came year after year from his pen, evidencing sustained interest and power. At the same time he became influential in the affairs of the University and an inspiration to its students. With Jordan he gave a course on evolution which aroused such interest and enthusiasm that numerous study groups were formed for more extended discussions. Here Kellogg grew and ripened and prepared himself unknowingly for the heavy responsibilities that were later to fall on his shoulders.

The final scenes of Kellogg's life were laid in places remote from those of his early life, and they were remote also in the character of the interests which they held. These interests were much more general and popular and their relations and implications more generally understood. The activities which they engendered made but small contribution to the development of his personality and character, which were well established when the world's madness called him to Europe to make application of the knowledge and training which the west had given him. Circumstances which he there encountered did, however, open up much greater opportunities for his talents, and doubtless greatly strengthened and broadened his purpose to make biology a force in human affairs. His success in alleviating suffering and in interpreting the motives and activities of contending peoples is well known and is evident in the honors that came to him.

Participation of the United States in the world war brought him at last to Washington to aid in the organization of science in support of the Government. The first formal result of these efforts was the organization of the National Research Council in which he became chairman of the Divisions of Agriculture, Botany and Zoology. When the Council was later made a continuing body he was named the Permanent Secretary, in which office he continued, active and emeritus, until his death.

He was also Chairman of the Division of Educational Relations for ten years and a member of innumerable committees Indeed he was the real integrating, and largely directive, force in the operation of this body which has done so much to make the scientific organizations of this country a working force. His conduct in all the important matters which came up for action was characterized by tolerance, good judgment and practical idealism. It is not too much to say that whatever success the Council has had is due largely to his activities. The limited space available makes a detailed enumeration of his achievements impossible, but they may be inferred from the list of offices he occupied.

If one were required to designate the most outstanding characteristic of Kellogg, he would unhesitatingly think of his intense and sustained activity—both mental and physical. His mind was constantly thrusting out in search of new ideas and contacts. This led him early into research—an interest which he maintained throughout his life, although in later years his response to more insistent demands did not permit its continuation. But always the pace was too slow when it depended upon the efforts of one individual and so he read much and widely. A considerable proportion of his bibliography is occupied with titles of reviews and critiques. This accumulation of information led to the production of numerous text books, and many newspaper and magazine articles. As his experience broadened the subjects of his discussions became less and less technical and more and more general. Practical applications of biology always interested him, but as the years passed and he saw more clearly the service which biology might render to social progress, this became the theme of his writings. To conceive a thought was to express it. His judgment, nevertheless, was remarkably good for one who wrote so readily and continuously. In time the ethical implications of scientific thought came to occupy much of his attention. As an example of this phase of his thinking an excerpt from his discussion of death may be given: "Death may possibly be not only that normal incident in human life we recognize it to be, but it may be simply one, the last one we now know, of a series of profound evolutionary changes in an

organism which has a continuing career of which we know now only the earlier stages; that is the stages of conception, embryology, adolescence, senescence and death.

"Death may not be the end, but simply another change in human life, greater and more radical, but perhaps no less possible than the changes from the single egg cell to myriad-celled and utterly different. Death may be but the change from one condition of humanness to another."

Instead of relying entirely upon my own judgment for a choice of the qualities in Kellogg which were most characteristic and significant, I consulted the opinions of others. Some of these are here recorded:

Mme. Jusserand: "I think of his splendid work, of his modesty and disinterestedness, of his eagerness to help his fellow men by his science and learning. And how could I ever forget what we owe him, here in France, for the lives he saved and for the sympathy he showed for our people in their hours of dire need. His intelligence, his heart, his tact made him succeed in a task where the lack of either would have meant failure."

Harold Heath: "From the outset he displayed a keenness of intellect, and a most active interest in literary and educational subjects, as well as in his chosen field, biology. * * * The interest created by these past masters (Jordan and Kellogg) in the art of presentation was great indeed. Discussion groups were formed in the student body and it is safe to say that the results exerted a profound influence upon many individuals and schools far beyond the confines of the Stanford campus. * * * It is safe to say that he exercised a lasting influence on the early life of Stanford University and was one of its great leaders."

Harlan Stone, Supreme Court of the United States: "To those of us who knew Vernon Kellogg best, his life presents a pattern of contrasts which are nevertheless singularly harmonious. * * * Scientifically trained, for most of his life a teacher of science, and never forsaking his scientific interest, he became more and more the guide, philosopher and friend of worthwhile educational and philanthropic undertakings.

"The eminent service which he rendered to science, to education, and the humanities, and above all the grace and integrity of

his mind, revealed to the stranger by his gifted pen, are the precious memories of those who knew him, and who, knowing, loved him."

Ray Lyman Wilbur: "He was characterized by a beautiful clearness and simplicity in thinking and statement. This combined with his appreciation of art, his dramatic sense and his broad human sympathy and understanding, made him one of our great authors of popular science."

Resolutions of the Academic Council, Stanford University: "* * * these words spoken in grateful remembrance of a charming friend, distinguished colleague, great-hearted and far-seeing citizen of the world."

Editorial, Washington Post: "Dr. Vernon Kellogg was one of those rare spirits, found most frequently in the scientific world, in whom unusual talent and unusual charm of character were most happily combined. * * * The man who is honored alike by scientists, by statesmen, and by little children is one whose contribution will endure. * * * So the name of Vernon Kellogg rests secure among those Americans of our day who have been of memorable service to humanity."

Editorial, Emporia Gazette: "But what he learned in journalism, indeed what he learned anywhere, he took with him. His life was an accumulation of ten thousand things that he had learned in passing through the wilderness of the world. So he was gentle, wise and kind to the end. * * * With all his learning, with all his wisdom, with all his gentleness, and with all the love he bore so many friends, also he had great courage."

CHRONOLOGY

1867. Born December 1, 1867, Emporia, Kansas, son of Lyman Beecher and Abigail (Homer) Kellogg.
1889. Graduated, A.B., University of Kansas.
1890-93. Assistant Professor of Entomology, University of Kansas.
1892. Graduate, M.S., University of Kansas.
1893. Student, University of Leipzig.
1893-94. Associate Professor of Entomology, University of Kansas.
1894-95. Assistant Professor of Entomology, Leland Stanford University.
1895-96. Associate Professor of Entomology, Leland Stanford University.
1896-1920. Professor of Entomology, Leland Stanford University.
1897. University of Leipzig.

1904. University of Paris.
1908. At Florence, Italy, married Charlotte Hoffman of Berkeley, Calif.
1908. University of Paris
1910. His daughter, Jean Kellogg, born in Berkeley, California.
1915-16. Director, Brussels, American Committee for Relief of the Belgians.
1917-19. Assistant to U. S. Food Administrator.
1918. Chairman, Division of Agriculture, National Research Council.
1918-21. Chief of mission to Poland, special investigator in Russia, member American Relief Administration.
1919-31. Permanent Secretary of the National Research Council.
1919-29. Chairman of Division of Educational Relations, National Research Council.
1919-34. Member of Research Information Service, National Research Council.
1919-34. Member, Division of States Relations, National Research Council.
1919-33 Member, Division of Foreign Relations, National Research Council; Vice-chairman, 1921-33
1921-33 Board of Trustees, Science Service
1925-31. Member of Executive Committee of International Research Council.
1931. Secretary Emeritus, National Research Council
1937. Died, August 8, at Hartford, Connecticut.

DEGREES AND MEMBERSHIP IN SOCIETIES

LL.D. University of California, 1919, Brown, 1920; Sc.D. Oberlin, 1922.

National Academy of Sciences.

American Society of Naturalists; American Entomological Society; Ecological Society; Association of Economic Entomologists; Genetics Association; American Philosophical Society; Washington Academy; Kansas Academy; California Academy; Academy of Natural Sciences, Philadelphia; Entomologische Gesellschaft; Société Éntomologique de France.

Officer of the Legion of Honor (France) Commander of the Crown (Belgium) Commander of the Order of Leopold I (Belgium) Commander of the Order of Polonia Restituta (Poland), Gold medal (Poland).

Trustees of Rockefeller Foundation, Brookings Institution, Gallaudet College.

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The extensive series of publications by Kellogg is impossible to list in its entirety in the limited space here available. Therefore only books and the more important scientific articles will be mentioned by name, while the total numbers of other classes of writings will be given. The range of subjects treated is most astonishing and rarely, even in hastily written articles, is there any lapse in style or scientific accuracy. When it is remembered that this extensive series of writings is but the by-product of a life full of teaching and administration, its extent and character are almost unbelievable.

In addition to the list of scientific papers, books and articles in books, here appended, there appeared book reviews to the number of 37 from 1920 to 1924; magazine articles to the number of 102 from 1916 to 1926; and newspaper articles syndicated, in many papers, to the number of 52 during the years 1920 to 1927.

EARLY OR MORE IMPORTANT ARTICLES

1895

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The mouthparts of the Lepidoptera. Am. Naturalist 5:546-556.

The affinities of the Lepidopterous Wing. Am. Naturalist 5:709-717.

(With F. J. Jack.) The California Phryganidian (Phryganidia californica Pack). Proc. Calif. Acad. Sci. ser. 2., 5: 562-570. Sept.

The Ephemeridae and venation nomenclature. Psyche 7:311-315 December.

1896

New Mallophaga. I. Proc. Calif. Acad. Sci. ser. 2, 6:31-168. March.

The Mallophaga Psyche 7:375-379. May.

New Mallophaga. II. Proc. Calif. Acad. Sci. ser. 2, 6:431-548.

Mallophaga of North American Birds. Zool. Anz. 19:121-123.

1899

Mallophaga from birds of Panama, Baja, California and Alaska. Occ. Papers, Calif. Acad. Sci. 6:1-52. February.

(With B. L. Chapman.) Mallophaga from birds of California. Occ. Papers, Calif. Acad. Sci. 6:53-141. February.

The mouthparts of the Nematocerous Diptera I-V. Psyche 8:303-306, 327-330, 346-348, 355-359, 363-365, January-June.

A list of the Biting lice (Mallophaga) taken from birds and mammals of North America. Proc. U. S. Nat. Mus. 22:39-100.

1900

Notes on the Structure and Life History of Blepharocera capitata Loew. Ent. News 11:305-318. January.

(With S. I. Kuwana.) Mallophaga from Alaska birds. Proc. Phila. Acad. Sci. 23:151-159.

1901

Phagocytosis in the Post-embryonic Development of the Diptera Am. Naturalist 35:363-368. May.

1902

(With B L. Chapman) Mallophaga from birds of the Pacific coast of North America. Jour. N. Y. Ent Soc. 10:20-28.

Development and Homologies of the Mouthparts of Insects. Am. Naturalist 36:683-706. September.

1903

The Net-winged Midges (Blepharoceridae) of North America. Proc. Calif. Acad. Sci. 3d ser. 3:187-232. February.

Two New Genera of Mallophaga Biol. Bull. 5:85-91. July.

Some Insect Reflexes. Science 18:693-696. November.

1904

Restorative Regeneration, in Nature, of the Star-fish, Linckia diplax (Muller and Troschel). Jour. Exp. Zool. 1:353-356. August.

(With R. G. Bell.) Variations induced in larval, pupal and imaginal stages of Bombyx mori by controlled varying food supply. Science 18: 741-748. December.

(With R. G. Bell.) Studies of variation in insects. Proc. Wash. Acad. Sci. 6:203-332. December.

Regeneration in larval eggs of silkworms. Jour. Exp. Zool. 1:593-599. 10 figs. December.

Influence of primary reproductive organs on secondary sexual characters. Jour. Exp. Zool. 1.601-605. December.

1906

Physiological regeneration in insects. Science 23:149-152. January.

Galls and gall flies. Nature Study Review 2: 109-114. March.

Mallophaga from Argentina. Journ. N. Y. Ent. Soc. 14:45-49. March.

Colors of butterflies and moths. Nature Study Review 2:206-211. September.

The scientific aspects of Luther Burbank's works. Popular Science Monthly 69:363-374. October.

Is there determinate variation? Science 24:621-628. November.

Variation in parthenogenetic insects. Science 24:695-699 November.

A second collection of Mallophaga from birds of the Galapagos and Revillagigedo Islands and neighboring waters. *In* Trans. Am. Ent. Soc. 32:315-324. November.

1907

Sex differentiation in larval insects. Biol. Bull. 12:380-384.

Artificial parthenogenesis in the silkworm. Biol. Bull. 14:15-22. December.

1908

The Mallophaga of the world. Systematic summary. Psyche 15:11-13. February.

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The Mallophaga of the Kilimandjaro Region. Wiss. Ergeb. Schwed. Deutsch.-Afrika Exp. Fasc. 15:43-56. 1 pl

1910

Is there determinate variation? Science 31:401-403. November.

1911

An experiment in double mating. Science 33:783-789. May.

(With J. H. Paine.) Anoplura and Mallophaga from African Hosts. Bull. Entom. Res. 2:145-152. July.

1914

Mallophaga from birds of the South Atlantic. Sci. Bull. Brooklyn Inst. Art and Sci. 2:80-89. November.

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BOOKS

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(With D. S. Jordan.) Evolution and animal life. 489 pp. 298 figs. 1907. D. Appleton and Co., New York.

Diptera, Family Blepharoceridae Fasc 56, Genera Insectorum, 15 pp. 1907. P. Wytsman, Brussels.
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Insect stories 298 pp. 1908. Henry Holt and Co., New York.
Internal anatomy of Diptera. *In* Williston's "North American Diptera." pp. 49-51 1908. Jas. T Hathaway, New Haven.
In and out of Florence: A new introduction to a well known city. By Max Vernon (pseud.) 370 pp. 1910. Henry Holt and Co, New York.
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Beyond war. 172 pp. 1912. Henry Holt and Co., New York
(With R. W. Doane.) Economic zoology and entomology. 532 pp. 245 figs. March, 1915 Henry Holt and Co., New York.
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Headquarters nights. 116 pp 1917. The Atlantic Monthly Press, Boston.
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Fighting starvation in Belgium. 219 pp. 21 illus. 1918. Doubleday Page and Co., New York.
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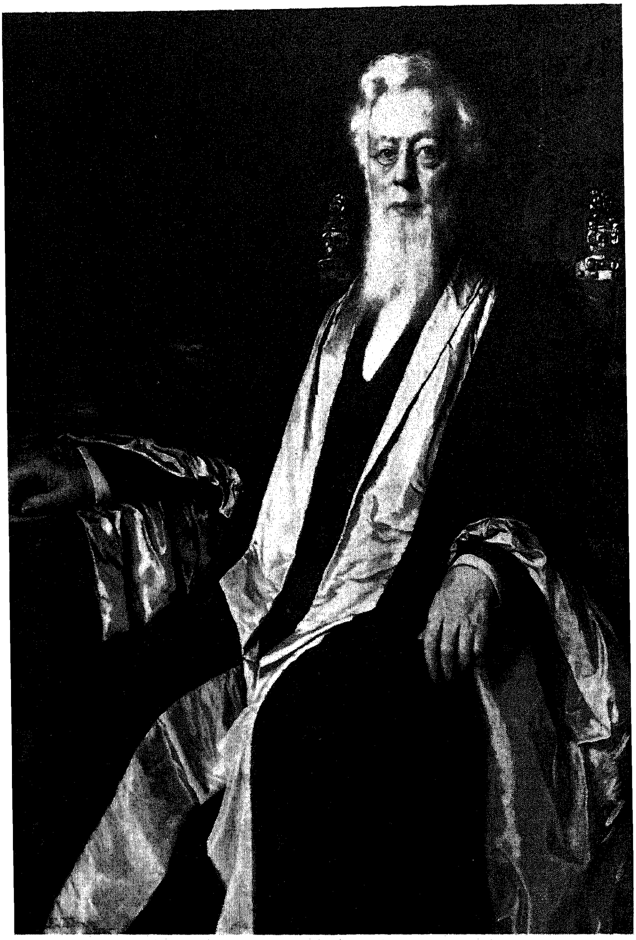

F. A. P. Barnard.

NATIONAL ACADEMY OF SCIENCES
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BIOGRAPHICAL MEMOIRS
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BIOGRAPHICAL MEMOIR

OF

FREDERICK AUGUSTUS PORTER BARNARD

1809–1889

BY

CHARLES B. DAVENPORT

PRESENTED TO THE ACADEMY AT THE AUTUMN MEETING, 1938

FREDERICK AUGUSTUS PORTER BARNARD

1809–1889

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There have been enough cases of brothers and fathers and sons elected to the National Academy of Sciences to support the view of hereditary genius. Besides Agassiz, Bailey, Dana, Draper, Lyman, Mayer (Mayor), Mendenhall, Silliman, Van Vleck, fathers and sons; there have been elected the brothers Hilgard, LeConte, Whitney and Compton. One of the striking cases is that of the Barnard brothers, John G. Barnard, one of the incorporators of the Academy,* and Frederick Augustus Porter Barnard also an incorporator, subject of the present memoir. John was engineer and mathematician; Frederick administrator and mathematician.

These brothers are stars of a famous galaxy. Their father, Robert F. Barnard of Sheffield, Massachusetts, was a lawyer of marked ability who was several times state senator. His father in turn was a physician, and a generation or two back we have military men and a physician. The mother of these brothers was Augusta Porter. Through her side of the house there were half cousins (1) Henry Porter Andrews (b. 1822) who was a civil engineer attached to the Engineer Corps, U. S. A., and helped John G. Barnard in his survey of the isthmus of Tehuantepec. He helped fortify the "Golden Gate" and New York harbor defenses, and was paymaster of the army throughout the Civil War. Also, (2) Henry C. Walton, graduated from Columbia School of Mines, a metallurgist of distinction.

* Of John Gross Barnard the Academy has published a biography. Born 1815, he studied at West Point, graduated second in a class of 43. He erected fortifications at Pensacola and New Orleans, was principal engineer in war with Mexico, superintended the defense at Tampico and battlefields about Mexico. He was chief of the survey of the isthmus of Tehuantepec for a route to the Pacific ocean; Superintendent of U. S. Military Academy 1855-56, and a leading engineer in the army during the Civil War.

His biographer states that he was "modest and retiring in disposition, considerate and courteous, warm in his sympathies, and his name will be cherished with peculiar love and affection by his brother officers. He had a keen sense of humor and a passionate love of music."

Of the brothers of the mother's father Joshua Porter was a surgeon in the Continental Army and first president of Saratoga Springs village; his nephew was a state senator in Connecticut. A brother of Joshua, Augustus Porter, was a land surveyor and had a son who became United States Senator. Another brother of Joshua was a Member of Congress before the war of 1812, became brevet major general in the war, and was Secretary of War in President Madison's cabinet. His son, Captain Peter, was killed in action in the Civil War. Others of this galaxy might be cited who were distinguished chemists, metallurgists and warriors.

Into a less desirable heritage the Barnards were born. There was hardness of hearing (otosclerosis) on the mother's side. She was affected and both of her sons.

Such was the family background of Frederick, born at Sheffield, Massachusetts, on May 5, 1809. His education was somewhat incidental till he attended Saratoga Academy while living with his mother's father at Saratoga Springs. After further schooling at Stockbridge he entered Yale College, 1824, and was graduated in 1828, standing second in the honor list.

After two years of teaching at Hartford, Frederick was called to Yale College as tutor. At that time each sub-senior class was divided into groups each of which recited all lessons to one tutor. Barnard inaugurated the reform of having tutors for each special subject. His was mathematics.

Growing deafness led him to accept a tutorship (May, 1831) at a Hartford school for deaf mutes and a year later at the New York Institution for the Deaf and Dumb. While at the latter institution he published an Analytical Grammar (1836) intended for the deaf, and prepared a paper on the aurora. In 1837 he was elected professor of mathematics and natural philosophy at the University of Alabama, and continued there for seventeen years, the last six as professor of chemistry. In 1854 he was called to the chair of astronomy and mathematics at the University of Mississippi at Oxford. Two years later he was elected to the office of President, later changed to Chancellor. At Alabama he built and furnished a small astronomical observatory and suspended a Foucault pendulum from a dome by a ninety-

foot piano wire. He invented stereoscopic photography. He took up journalism, sometimes writing political editorials for the two newspapers of opposed principles, and on occasion refuting his own editorials. He served as a commissioner to relocate the boundary between Florida and Alabama. He took a leading part in discussions as to university policy. In 1854 he received orders in the Episcopal church.

At Oxford, Barnard built up a strong institution, secured the erection of astronomical and magnetic observatories and ordered a nineteen-inch lens from Clark of Cambridge; but as this was not completed before the Civil War was declared it was never delivered to the University of Mississippi, but to the Dearborn Observatory at Chicago.

In 1860 Barnard accepted an invitation of A. D. Bache to accompany a total eclipse expedition to Labrador. He returned to Newport, R. I., to find that he had been elected president of the American Association for the Advancement of Science. On account of the war the next meeting was not held until 1866, so that Barnard holds the record for length of office of president of the Association.

Barnard was a northerner, though a slaveholder, caught in the South at the outbreak of secession, of which he disapproved. Almost all the students entered military service and he resigned his office as Chancellor. The trustees of the University begged him to withdraw his resignation, which he did conditional upon the reopening of the University in the autumn. Such reopening did not take place so Barnard left with the good will of the trustees and a commission to report to them on military schools in South Carolina and Virginia. He made the report in person. President Jefferson Davis of the Confederacy personally urged him to stay in the South as he was needed to direct the work of obtaining sulphur from the mines of western Tennessee, but he declined and went with his wife to live at Norfolk, Virginia, until that city was captured by Federal troops in May, 1862.

Coming to Washington he was given direction of the map and chart department of the Coast Survey under A. D. Bache. This included the preparation and publication of war maps. While thus engaged he published his famous "Letter to the President

of the United States by a Refugee," which denounced slavery, the "giant conspiracy" of southern leaders and especially the work of northern Copperheads as the greatest danger faced by the Republic. Shortly after this letter appeared Barnard was elected tenth President of Columbia College, in 1864, at the age of fifty-five.

Barnard entered upon his work with energy and tact. He had to revive a feeble School of Mines. In his inaugural address, at a time when the conflict between science and religion was being much discussed, he took for his topic the real absence of such a conflict. As a priest and a man of science he sought to harmonize the two camps.

During the years of his presidency Barnard adhered closely to his duties of building the college into a university. He was appointed by the President of the United States on a government commission to the Paris Exposition of 1867 and to the exposition at Vienna, at both of which he was on a committee on instruments of precision. He made four other summer trips abroad, being everywhere received as a distinguished American citizen.

In the field of education he took a pleading part. As President Butler says: *

"Among the new visions which President Barnard had during his quarter century of service as administrative head of Columbia College were: the elective system of undergraduate study and the enriching of the undergraduate curriculum; the reform of the examination system; the emphasis which should be placed upon the study of modern European languages; the building of a university organization after the fashion of those of continental Europe upon the foundation of the undergraduate college; the provision of opportunities for the higher education of women, equal in all respects to those provided for men, this to be accomplished either through co-instruction of young men and young women in the same institution or by the establishment of separate colleges for women; the study of education as a science and the development of a plan for the professional training of teachers which should take its place side by side with plans already existing for the professional training of lawyers and physicians; and finally, the larger service of college and university to the general public which has since found expression in University Extension, in Home Study, and in various other forms of carrying the fruits of contemporary scholar-

* The Rise of a University. Vol. 1. Columbia Univ. Press. 1937.

ship to great companies of eager men and women who are no longer formal students at any institution."

As President of the College Barnard issued annual reports in which he set forth his views of education in general and the achievements and needs of the College. These writings have been republished in "The Rise of a University" (Vol. 1, 1937), by the Columbia University Press. They cover the whole field of education. As a man of science he early (1879) called for the need of provision of graduate instruction. It seems remarkable to us today that in 1882 the biological sciences were "all unrepresented in our scheme of instruction" at Columbia. He early urged that Columbia should follow Harvard in the adoption of the elective system, but he later came to see that undergraduates would be aided in choice of electives by the advice of a member of the faculty.

In May, 1888, Barnard presented to the Trustees of Columbia College his resignation as President. He was now in his eightieth year and his health unstable. These facts led to the acceptance of his request. He lived less than a year longer, dying April 27, 1889. In his will, being childless, he left a fund to the College for "encouraging scientific research." Also a fund for the increase of the library. He made provision for a gold medal to be awarded every five years to the person who shall "have made such discovery in physical or astronomical science or such novel application of science to purposes beneficial to the human race as, in the judgment of the National Academy of Sciences of the United States, shall be esteemed most worthy of such honor." Among recipients of this medal have been Niels Bohr, Sir William Henry Bragg, jointly with his son William L. Bragg, Albert Einstein, Warner Heisenberg, Edwin Hubble, Ernest Baron Rutherford.

Frederick Barnard, a scion of a family of professional men, lawyers, statesmen, physicians, military men, chemists, engineers and mathematicians, originally trained for the law, was led, on account of a family defect in hearing, into teaching and administration. He maintained chemical and astronomical research as an avocation.

Barnard was about six feet tall and in his later years grew a long white beard and reminded one of his former students of the conventional pictures of Moses. From youth he was gay in disposition and had an attractive personality. When in Hartford he arranged to go to a concert with some of Miss Beecher's girls when they should have attended one of her "exhibitions"; but the plot was discovered in time to foil it. From an early age he showed a somewhat non-plastic disposition. Thus when at school the tutor called him to account for some offence that Barnard did not regard as such, and demanded an apology, the boy refused and was publicly censured before the whole school. The Board of Trustees of the University of Alabama had voted unanimously a certain plan for instruction, Barnard wrote a full report opposing that plan and gained a partial victory. In later life at Columbia College he showed an intolerance of opposition and a certain imperiousness of manner. He had something of the warrior traits that were part of the family heritage.

Barnard wrote easily. He wrote many pieces of poetry, edited papers while at the University of Alabama and contributed to a literary journal. His exhaustive report on a proposition to modify the plan of instruction in the University of Alabama was written, while still busy with his usual college work, within six days. At his inauguration as Chancellor at Mississippi he wrote a long "Open Letter" to the Board of Trustees, urging scientific studies. His "Letter to the President of the United States" was an effective if somewhat exaggerated statement. His annual reports at Columbia were distinguished by fullness and clarity and had an immense influence. From 1873 to 1877 he was engaged in heavy literary work as Editor in Chief of Johnson's Cyclopedia containing 7,000 closely-printed pages; for which he wrote many important articles. In his last year he wrote much autobiographical material.

In speech he was equally a master of words. As a recent college graduate he made a Fourth of July speech. In 1851 at Alabama he again delivered a public oration; for "his eloquence was universally admired."

Barnard had variable moods. At twenty years while teaching, he would sit for a half hour at a time with his head bowed on

his desk, and his gloom was deepened by the incidence of deafness. At other times his gaiety was extreme.

Of him President Nicholas Murray Butler says:

"His activities were gravely limited and conditioned by his very severe deafness. In his office, he had standing on his desk a large wooden sound receiver, perhaps two feet square, into which one spoke when conversing with him. When himself speaking, his voice was naturally affected by his deafness and was neither clear nor pleasant. On the other hand, he had great charm of personality, manifested by his facial expression, by the character and cordiality of his speech and by his generous and kindly interest in those with whom he was associated.

"As a matter of fact, I owe to Dr. Barnard not only the choice of my career, but the determination to stick to it despite every sort and kind of temptation, whether financial or political."

Professor James C. Egbert of Columbia writes:

"I remember him very well indeed. He was a tall man with a long gray beard, very reverend in appearance, very dignified. I remember well how he conducted the commencement exercises, speaking most distinctly and appropriately. He was very deaf, and his desk was fitted up with a sort of amplifier through which the person who was calling upon him was compelled to speak. This was very awkward when one was particularly anxious to make an impression on him. I do not think that he was a man of ready temper; I should say that he was placid and not easily provoked. As I have suggested above, he had a most dignified bearing, especially in the presence of the students. Those who were interested in their work were always received readily by the President. I remember very well the letter which he wrote for me when I was thinking of seeking a position as a teacher. He was most kindly and fair in this statement, the sort of letter of recommendation which would have satisfactory effect. You will see that these statements which I have given are personal and indicate the impression which President Barnard made upon me. Many of us feel that President Barnard was most progressive in education and really had foresight as to the sort of institution Columbia should become and could become."

The following extract from the minutes of the Alumni of Columbia University give a contemporary estimate of President Barnard:

"In 1864 at the date of Doctor Barnard's accession to the presidency, the College was at a critical period of its history. It was ready for development and had begun to develop. The Law School had been established a few years previously and was in successful operation. The School of Mines was in process of organization. The Trustees had for several years

been considering the expansion of the undergraduate course, and in connection therewith a system of university education. At this critical period the College happily obtained as its chief counsellor and guide Dr. Barnard, a profound student of education, in sympathy with all the forms of higher development, literary as well as scientific, of quick perception, peculiarly open to new ideas and prolific of them, of learning deep, exact, and extensive in many fields, a classical and English scholar, a fine mathematician, physicist, chemist, and adding to his severer accomplishments that of being a poet and a musician of no mean quality, a prolific, elegant, and persuasive writer, a logical and convincing speaker, of sanguine enthusiastic temperament, bold and persistent in the advocacy of his opinions, and impervious to discouragement. He quickened into organic life the School of Mines, he gave vitalizing force to the extension and liberalization of the undergraduate course, to the founding of fellowships for the encouragement and assistance in their higher studies of earnest and able young men, to the extension of the library and the liberalization of its management, to the project of a course for the higher study of political and historical subjects, and to the scheme for a broad and liberal system of post-graduate or university instruction, which the College had long but vainly desired. In brief, he gave Columbia College a new life and a new significance, and by his commanding position in many learned societies, by the force and elegance of his published writings, scientific, literary, legal, political, educational, and by his wide acquaintance with the foremost men of his time, he attracted attention to the College, and did much to interest the community at large in it.

> Age could not wither nor custom stale
> His infinite variety.

He possessed, with such men as Gladstone and Bismarck (it is a very rare quality) the fervor in age that he had in youth, and was as ready as he was before he had secured position and fame, to take up a new idea, a new project, and pursue it with as much vigor as if a long life were still before him, and all his reputation yet to make. It was this quality that made him a great president to the very last. With almost his latest breath, unable to write, and speaking with difficulty, he dictated letters of counsel upon what was ever nearest his great heart—Columbia College and her future."

His scientific work was an avocation. On present day standards he might be regarded as a very gifted man whose other interests left him little time for fundamental work in science, although in photography his use of chlorine gas and invention of stereoscopic photography were real achievements. His knowledge and accuracy in using the sextant made him indispensable in locating the correct state boundary line. Again, in the Coast

Survey his work in preparing war maps required accuracy. He was in his day the foremost propagandist for the metric system in the United States. His analysis of the theory of magic squares reveals his hereditary mathematical genius. The examination of his bibliography, appended to this paper, gives the best idea of his scientific productiveness.

Barnard adapted himself well to any situation in which he found himself. While in the South there is no evidence that he entertained any strong feelings on slavery. Says Fulton, "He was not a man whose feelings governed his convictions." He accepted slavery as an unwelcome fact; and of his own will he became a slaveholder. After knowing him twenty years two southern gentlemen testified that they "had never heard his attachment to the institutions of the South called in question." He himself stated to his board, "I am a slaveholder and, if I know myself, I am sound on the slavery question." Yet after he had passed the war barrier and was safe in Washington, in the circle of abolitionist friends, he denounced "that relic of primeval barbarism, that loathsome monument to the brutalities of the ages of darkness, that monster injustice—cursed of Christian men and hated of God—domestic slavery."

The outstanding traits of Barnard, those that made him great, were the broad sweep of his imagination and his vision of the future, combined with the special gifts of administration and others that might have made him a noteworthy engineer. These, together with his capacity for making and holding friends and commanding support for his ideas, made him one of the great builders in the field of education.

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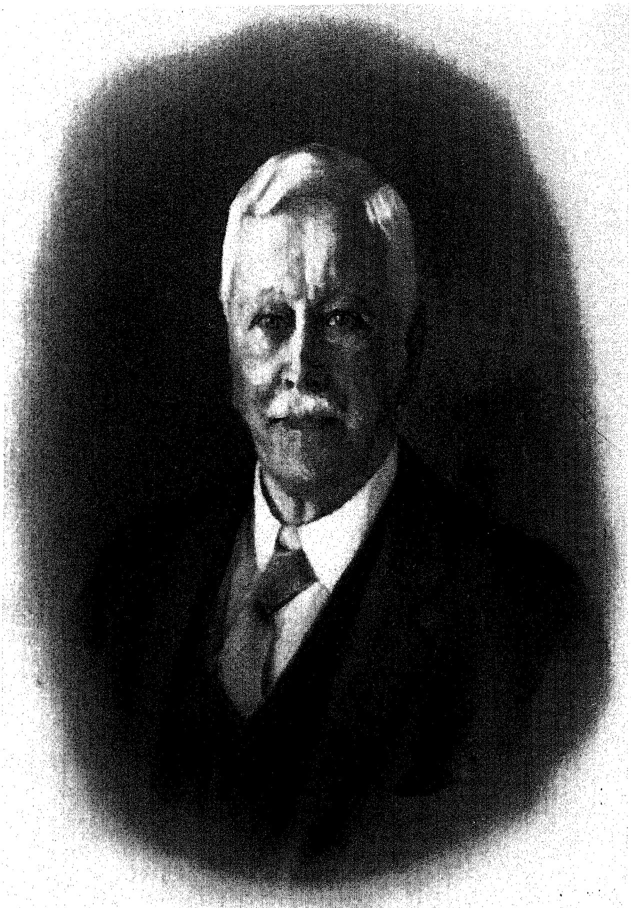

Charles S. Hastings

NATIONAL ACADEMY OF SCIENCES
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BIOGRAPHICAL MEMOIRS
VOLUME XX—ELEVENTH MEMOIR

BIOGRAPHICAL MEMOIR

OF

CHARLES SHELDON HASTINGS

1848–1932

BY

HORACE S. UHLER

PRESENTED TO THE ACADEMY AT THE AUTUMN MEETING, 1938

CHARLES SHELDON HASTINGS

1848–1932

BY HORACE S. UHLER

Charles Sheldon Hastings was born at Clinton, New York, on November 27, 1848.[1] His New England ancestry included an unusual proportion of professional men, particularly physicians. One of his great grandfathers, Dr. Seth Hastings, was born at Hatfield, Massachusetts, in 1745. After transferring his home to Washington, Connecticut, an eldest son was born to him in 1780 and given the name Seth, Jr. This grandfather of the subject of the present memoir subsequently moved to Clinton, New York, where he also practiced medicine and became, in the year 1816, the father of Panet Marshall Hastings. The latter graduated from Hamilton College at the age of twenty two, became a very prominent physician in Clinton, and gave lectures on anatomy and physiology at his alma mater. In the year 1843 Dr. P. M. Hastings married Jane Sheldon, a lady whose forebears were likewise sterling representatives of New England. About five years later this couple became the parents of Charles Sheldon, and when he was approximately six years old they changed to a permanent residence in Hartford, Connecticut.

In this city Hastings received his early training in the public schools and passed from the Hartford High School to the Sheffield Scientific School of Yale University in the fall of 1867. From this Institution he received the degree of Ph.B. in 1870 and then continued as a graduate student for a period of three more years. The diploma of Hastings' doctorate bears the date of the 26th of June 1873. During the last two years of his enrolment as a graduate student he held the position of Instructor in Physics in the Sheffield Scientific School. He then resigned in order to devote the next three years to study and travel in Europe. It was his inspiring privilege to attend

[1] Biographical notices of C. S. Hastings have been published by Frederick E. Beach and Frank Schlesinger. The respective references are: *Amer. Jour. Science,* 23, 485 (1932) and *Astrophys. Jour.,* 76, 149 (1932). With one exception, for which acknowledgment is made in the proper place, the present memoir owes nothing to these earlier papers.

courses given in Berlin by H. L. F. von Helmholtz and in Heidelberg by G. R. Kirchhoff. It may be interesting to record that Hastings' note-books and scattering memoranda indicate that he took Kirchhoff's lectures on optics which commenced on the 24th of April 1874, that he studied advanced mathematical analysis under Professor Königsberger, that he visited Steinheil's on March 11, 1875, and that in Paris during November 1875 he was buying scientific books as requisites to the courses at the Sorbonne. The sojourn in Paris was facilitated by the fact that Hastings was awarded the "Tyndall Scholarship" for the year 1875.

In the next year an event of paramount importance to the development of higher education in the United States of America and to the advancement of scientific research in the world occurred when the Johns Hopkins University was launched in Baltimore, Maryland. At the beginning there were appointed to this faculty—under the sagacious selecting by the first President of the University, Daniel Coit Gilman—six professors and seven "associates". In the list of associates, many of whom became leaders in their several departments of study, the name Charles Sheldon Hastings deservedly appears. Not one of these associates had attained the age of thirty years.

Hastings' academic title was first changed in the fall of 1882 to "Associate in Physics, Sub-Director of the Physical Laboratory, and Lecturer on Solar Physics." In 1883 the first three words of the title just quoted were replaced by "Associate Professor of Physics." He then resigned to accept the call to occupy the newly entitled [2] chair of Professor of Physics in the Sheffield Scientific School, New Haven, Conn.

Before passing to Hastings' career at Yale University the following fact merits presentation because it is presumably virtually unknown and it seems to be historically important. The fact is that the fields of applied optics, physics, and astronomy came very near losing, for a time at least but probably forever, the invaluable services of Hastings. In a notebook containing the long-hand manuscripts of several scientific

[2] Information relative to the founding of chairs in the Sheffield Scientific School may be obtained from the *National Cyclopaedia of American Biography*, vol. I, p. 172.

papers, most of which appeared later in print, there is to be found a letter simply indexed: "Letter to Scudder."[3] The full import of the matter may best be inferred from the letter itself which reads:

"Dec. 25, 1882.

"Your most flattering invitation to associate myself with you in editorial work has given me much more anxious thought than I had anticipated. At first I was strongly inclined to accept your offer, and had no doubt that the week granted me for consideration (for I supposed that an answer was not looked for before last Wednesday) would prove quite sufficient. At the end of that time, however, I was more uncertain than before, for this singular reason: I found that of the seven or eight friends whom I had consulted and whose judgment/opinions seemed to me of more weight than my own, all who were of nearly my own age advised me to go, whereas those who were considerably older strongly advised my remaining for the present in my position.

"The argument as urged by the latter was this: I am in a position which gives congenial work for which I have shown aptitude, and, although it is not such as ought to satisfy the ambition of a man of mature experience, it is one which yields its possessor valuable knowledge and secures him a constantly increasing number of friends. Such a change as contemplated means a change of work which, however kindly may be the judgments of my friends, I am by no means certain to prove well adapted, and from which an agreeable escape, in case the experiment were found to be a failure, would not be easy.

"The force of this reasoning must be granted, and I feel myself impelled to act upon it. In doing so my greatest regret is that I lose the opportunity of justifying in your own mind the gratifying things you have said to me and of me."

The following note written on the back of a list of physical apparatus then constituting the collection of the Sheffield Scientific School was doubtless very welcome and encouraging to the addressee.

"New Haven, Dec. 18, '83.

"My dear Hastings

"Here is the lean list of our apparatus. I am glad to know that $1000 has been put at your disposal by our Trustees to improve it. Mr. Richards, as you may know, has accepted, and it is a matter of course that both of you will be confirmed by the Corporation; so we are all well pleased. As ever

"Truly yours

C. S. Lyman"

[3] Possibly Horace Elisha Scudder.

If Hastings needed a rest in the sense of a complete change of mental stimuli and physical environment the voyage to Caroline Island to observe a total eclipse of the sun came in most opportunely. The American Party, of which he was a responsible member, sailed from New York on March 2, 1883, crossed the Isthmus of Panama by train, and finally arrived at the coral island on Friday, April 20th. From Sunday March 11th until the date just given Hastings wrote a very interesting and instructive diary of his impressions and experiences, especially those which were received or occurred on the trip across the Isthmus and at the successive points of call: Buenaventura, Tumaco, Guayaquil, Pata, and Callao. The group of scientists arrived in San Francisco on June 11, 1883 after having been absent from the United States for one hundred and one days during which about 12,300 miles had been traveled, and fifty days had been spent aboard the U. S. S. *Hartford*, Admiral D. G. Farragut's flagship in the memorable battle of Mobile Bay.

The salient features of Hastings' progress having now been traced from the time of his birth until he became permanently settled in New Haven, Connecticut, as Professor of Physics in the Sheffield Scientific School, attention will be turned in succession to the three chief aspects or phases of his life. These may be conveniently designated as: his optical researches, his character as a teacher, and his fitness relative to his social environment.

It is difficult to state exactly when, or in what way, Hastings' interest in astronomy and optics was first strongly aroused. His early and lasting liking for botany, geology, and zoology was probably initiated and fostered by his companionable father who was also a natural philosopher in the original general sense of this term. Undoubtedly Hastings' ever increasing devotion to astronomy during his undergraduate years was largely due to the influence of Chester Smith Lyman whose academic chair in the Sheffield Scientific School included both physics and astronomy from 1871 to 1884. A single sheet of paper dated October 1, 1869 gives a brief account of the telescopic observations which Hastings had just been making, also one sketch of the rings of Saturn and another of the surface markings of Jupiter, and a diagram of the relative positions of four of the

satellites of the largest planet. This was recorded near the beginning of his senior year in college.

Less than ten years later his skill in making lenses and his keenness of observation were attracting attention outside of Baltimore. This is attested by the fact that the prominent astronomer, S. W. Burnham, wrote Hastings a short letter from Chicago, Illinois, on July 2, 1879, the closing paragraph of which reads: "I have heard something of your glass from Mr. Rockwell. I hope you will follow the thing up, and if it proves to be a success as I have no doubt it is, try it on a larger scale." (The objective referred to had an aperture of 4.1 inches.)

That Hastings did "try it on a larger scale" with extraordinary success is now an accepted fact of scientific history. Since Professor F. Schlesinger was Director of the Allegheny Observatory of the University of Pittsburgh from 1905 to 1920 he was in a position both geographically and by virtue of his special field to observe with interest, and to write authoritatively on, that which may be called for brevity the "Brashear-Hastings-McDowell Association." For these reasons, and with the freely-given permission of its author, the following quotation is made.[4]

"In the late eighties he [Hastings] received a letter from a correspondent in Pittsburgh, at that time unknown to him personally, which was to prove of the greatest importance in shaping his career. A few years before, John A. Brashear and his son-in-law, James B. McDowell, had started the ambitious project of establishing their optical factory, an undertaking that would have been altogether impossible in that day in this country had it not been for the moral and financial backing of William Thaw. After some difficult years this venture prospered and was soon standing upon its own feet. Its prosperity brought with it the necessity for a mathematical expert to take care of the demands that the growing sciences of astronomy and astrophysics were making upon the ingenuity of these opticians. Brashear put this problem to his friend Professor George F. Barker of the University of Pennsylvania, who suggested that he secure the co-operation of Hastings. Brashear wrote at once to this effect and Hastings accepted. This was just as it

[4] Frank Schlesinger. CHARLES SHELDON HASTINGS. *Astrophys. Jour.* **76**, 150-151 (1932). As supplementary reading reference should be made to JAMES B. McDOWELL—AN APPRECIATION, by J. S. Plaskett, Director of the Dominion Astrophysical Observatory. *Jour. Roy. Astron. Soc. Canada*, **18**, 185-193 (1924). The frontispiece shows both Hastings and McDowell in characteristic poses.

should have been. On the one hand, it gave Brashear and McDowell the technical advice without which they could hardly have developed as they did; and, on the other, it gave Hastings precisely the clinic he needed to put to use his then unrivaled skill and knowledge in matters optical.

"These three men remained associates until the death of Brashear in 1920 and that of McDowell in 1923. Their alliance produced among other large instruments the 72-inch reflector at Victoria, the 30-inch Allegheny photographic refractor, the 26-inch Yale photographic refractor at Johannesburg, the Swarthmore 24-inch visual refractor, and the Keeler reflector at Allegheny with all its complicated auxiliary apparatus. They have also provided observatories with many wide-field cameras, including the Bruce doublet for Barnard at the Yerkes Observatory and the twin 16-inch Bruce Camera for Max Wolf at Heidelberg. Almost all the many spectrographs that were installed in American observatories in the early years of this century owe at least something to Hastings' design, and some of them were built entirely by this firm. Spectrographs attached to visual refractors necessitate a correcting lens between the main objective and the slit, and these Hastings computed with great success. For the Allegheny refractor the writer put the converse problem to Hastings, namely, to design an auxiliary lens that would transform the color correction from that of a photographic telescope to a visual, without sensibly changing the position of the focal plane. This, I think, was the most strikingly successful achievement of Hastings and McDowell: they provided a 12-inch corrector which is interposed nearly halfway up the tube and which gives visual images that I defy any observer to distinguish from those obtained directly by a visual objective of the highest quality.

"Among the many other optical problems that engaged Hastings' attention may be mentioned the cause of the various types of solar and lunar halos, the design of an Aplanat magnifier (which has earned him the gratitude of scientific workers in many fields and in all quarters of the globe), better correction for color by the use of two special types of glasses or by three ordinary types, and the optical faults of the human eye."

Toward the end of his life, but definitely before his memory had practically vanished, Hastings was devoting all of his working hours and by far too much of his energy to the design of microscope lenses. At this time he designed and made a 10× ocular which (in his own words) "has an absolutely flat and rectilinear field. Theoretically it is superior in definition to the accustomed 10×, and some of our expert microscopists assert that it is so." On April 24, 1930 he finished making with his own hands and testing an incomparably fine objective of 16 mm focal length,

of numerical aperture 0.3, and consisting of only three discrete lenses. The memorandum written the next day reads: "The objective was finished yesterday and is now about as good as I can make it without beginning all over again. It still has a minute error of excentricity both in back and front. It requires ocular 25× to exhaust its powers and it will bear 30× very well. The most difficult object which I have succeeded in resolving (with dark field illumination) is Pl. Balticum (38000 lines per inch according to Van Heurck)."

Whenever Hastings achieved a material optical triumph he naturally exhibited the apparatus with justifiable pride to some of his friends and colleagues. In this instance his enthusiasm was so great that he assured me that his new lens system was at least one hundred per cent better than anything of its rating then on the market. In order to obtain an independent opinion on this matter I recently made a point of visiting a friend who has had much experience in studying microscopic objects. Although about eight years had elapsed since he made observations with Hastings' best microscope, he recollected the circumstances vividly and said that the exquisite details brought out by this optical system exceeded to such a degree anything which he had ever seen that it seemed as if a whole new world had been unveiled to his vision. Be this as it may, it should be stated, in behalf of unbiased scientific accuracy, that none of us made crucial quantitative tests of these lenses.

The slight residual imperfections in the lenses would assuredly have been entirely removed both theoretically and practically if Hastings could have found the kind of manual help and intelligent cooperation which he had become so accustomed to receiving from McDowell. He did make an appeal for experimental aid but apparently nothing was vouchsafed him. Hastings frankly confesses: "My skill in lens making is limited. Surfaces of short radii and plane surfaces I can manage very well, and, less satisfactorily, concave surfaces of long radius, but convex surfaces of long radius give me a lot of trouble to avoid zonal errors." The lenses in question have disappeared but the work-sheets which contain all of the data and calculations have been jealously preserved.

One thing about Hastings which has not been emphasized sufficiently is his general scholarship. He was really a scientific philosopher—a scholar of broad and accurate attainments. He was an excellent physicist, thoroughly versed in his specialty and fully conversant with the physics of his day. But he was more than that. He paid no little attention to the philosophical implications of science and to its cultural values. His asides during lectures on historical developments in physics and related subjects, the reasons for them and their significance, scientific and other, were always illuminating and consequently interesting and valuable. He nearly always taught more than just the topic he happened to be discussing at the moment. This also made the matter he was presenting stick more firmly in one's mind. Instead of being an isolated fact to remember, it was part of a connected whole which was manifestly incomplete without it. These characteristics were especially marked in his advanced course on optics which I attended as guest in the spring of the year 1911. In particular the influence of Helmholtz was quite apparent and it led the lecturer to say in substance that a thorough study of the eyes of vertebrates would constitute in itself an excellent course in optics.

Hastings was eminently successful as a teacher of undergraduates. His material was wisely chosen and carefully prepared in logical sequence, and the demonstration experiments always worked perfectly because of his unusual dexterity. An interesting sidelight on the reactions which Hastings aroused in undergraduate students in the first years of his teaching at the Johns Hopkins University is afforded by the following quotation from a little book written by Allen Kerr Bond, M.D., entitled *"When the Hopkins Came to Baltimore."* (The Pegasus Press, 1927.) Incidentally Doctor Bond was the second undergraduate to be examined for admission to the Collegiate Department.

"The instructor in Physics, Dr. Hastings, had a seraphic smile which appeared only when one of his pupils at the blackboard was heading for a fall. When we saw it break out over his face, we sure knew that Trouble was waiting for us around the corner. He was the only teacher I ever had who defamed Spelling. He said he had wasted endless hours learning spelling, which now-a-days he left entirely to the proof-reader, as beneath his own notice.

"He was very expert in lens-grinding and made the fine lens of the telescope in our Academy of Sciences on Franklin Street.

"It was at his Physics class that I first heard a phonograph. One day he brought in a machine which he said had been made for him in a little shop on Eutaw Street, where an expert mechanic was employed in making models of inventions such as Professor Rowland's spectrum gratings and things needed for demonstrations. He set the machine a-going and it related to us the tragic story of Jack and Jill,—ending with 'Jill came tumbling after *him*.' 'I know,' said Dr. Hastings, 'that the apprentice boy spoke that record; for that is the way he always recites it.' "

A few comments on the text-book known almost everywhere as "Hastings and Beach" should be made because at the time of its publication (1898) it undoubtedly set a standard above anything before it in this country. The book is difficult chiefly because of its vigor and the amount of ground which it covers with very brief discussions in general. Hence it was an excellent text for students with good heads and a bad one for those with poor heads—excellent for those who wanted to know physics as physicists and engineers but poor for those who desired to learn something about physics as part of a general education. A copy now before me contains an inscription which speaks eloquently for one class of head.

"If we should have another flood·
"For safety hither fly.
"Although the earth would be submerged
"This book would still be dry."

Another book by Hastings—the mere existence of which is apparently known to only a few specialists—deserves consideration in this memoir because it .is potentially the guarded answer of its author to the oft repeated suggestion of fear on the part of his intimate friends that his method of designing lenses might be lost to posterity because most, if not all, of his research papers in this field give results and not specific directions. The title is *NEW METHODS IN GEOMETRICAL OPTICS with Special Reference to the Design of Centered Optical Systems.* (The Macmillan Co., 1927.) The opinions expressed below concerning the character of this volume should be nearly correct because they are based on actual experience with its contents in two different ways. In the first, a general

but rather superficial view was obtained by reading the page proof. In the second, I used the book as a text in the next giving of the first part of my graduate course on geometrical optics and the theory of spectroscopic apparatus. Although the book was not particularly designed for class use, I was surprised and disappointed to find that the pedagogical experiment did not succeed. The chief reason seemed to be that the material presented had been extremely condensed. After finding that it usually required from six to eight hours of computation and preparation on my part to discover precisely how Hastings had obtained even one of his illustrative numerical tables the conviction became gradually forced upon me that, aside from the easy purely mathematical analysis,[5] the text was essentially a compilation of results which its author had accumulated during his life-long experience as consultant and theorist for the John A. Brashear Optical Co. Nevertheless it should not be inferred from the preceding critique that this text is useless, rather it is "necessary but not sufficient", to borrow a mathematical phrase which fits the case admirably. The Rosetta stone is to be found in Hastings' work-sheets which give implicitly the trains of thought followed while repeating the calculations (with only a single card of four-place logarithms) until the errors of the centered optical system under design were made either to vanish or at least to satisfy the required theoretical tolerance. In other words, if Hastings had supplemented his text by including a long appendix exhibiting *all* of the tedious arithmetical labor which he patiently performed in the case of any one compound lens, then a properly prepared reader could find out precisely how to copy, continue, and perhaps extend the master's theoretical designing. Obviously this would not remove the necessity for a great deal of concurrent laboratory work.

A careful study of the available records showed that no graduate student has ever attempted to obtain one of the higher academic degrees by doing his thesis work under Hastings' guidance and in the special field of the latter. This fact may be

[5] Hastings uses the marks $\rfloor$ and ! in his original paper (1893) and in this book, respectively, to denote a continued product and not a factorial. This may lead to confusion because the accepted mathematical symbol is capital pi.

explained on the basis of the following deterrent influences: the drudgery and consumption of time inherent in the computations referred to above, lack of experimental facilities and especially optical glass, and above all the unsympathetic attitude which Hastings invariably took whenever he was asked a direct question about the practical application of the mathematical theory which he gave in his advanced course in optics. This selective taciturnity may have been accidental or temperamental but since, to his intimate friends at least, he was without exception a very approachable person it seems far more probable that the trait was constructively developed for defensive purposes.

Before considering Hastings' personality and social relations a few "fragments" gathered from his loose sheets of paper and note-books will be recorded because some of them may be of interest and probably not one occurs elsewhere in print. Considerations of brevity and of continuity of thought precluded the possibility of incorporating them in the preceding pages.

There exists the original white-on-black drawing by Hastings of satellites I, III, and IV "of Jupiter as seen Sept. 1, 1869." This is accompanied by a printed proof which implies publication. All endeavors at finding the reference have failed. The date corresponds to the beginning of his senior year.

Hastings early discovered an object in the constellation of Taurus to which he refers as "my double star". His first extant memorandum is dated Friday, Oct. 1, 1869. "New double in Taurus divided or rather disks just in contact with 450 solid." [Meaning solid ocular and magnification 450×.] "Compared it to γ^2 Androm. and it seemed a little closer and more difficult on account of faintness." "The components are of nearly equal size." On Feb. 2, 1882 Asaph Hall wrote a letter (from the U. S. Naval Observatory, Washington, D. C.) to Hastings in which he said: "Your star is a fine double. Last night I found $p = 298^\circ\!.3$: $s = 0''\!.54$ the night was only middling; and on a good night it would be an easy object here. When and with what glass did you find it?" On Feb. 11, 1882 A. Hall gave the magnitudes of the components as 8½ and 8.

Oct. 10, 1880. "Compared Steinheil's triplets very carefully

with my solid eyepieces of nearly equivalent powers. I could find no inferiority in point of definition or light, though the available field of mine is about 20% greater." Oct. 20, 1880. "New ¾ in. and 3/10 in. solid eyepieces; they are very good."

"The glass circle spectrometer seems to have been completed April 22, 1886."[6]

March 2, 1889. "Finished this day the 2¾ in. objective . . ." "It performs beautifully on all kinds of objects . . ." "The following observations were made to test the power of a telescope of 2¾ in. aperture and 38 in. focal length with perfect color correction. The comparisons are made with a 2⅝ in. telescope of 33 in. focal length (the first telescope that I made) which is constructed on the Herschelian type and very admirably corrected." This first telescope antedates Oct. 8, 1880 if it is the same one referred to on that date in the following quotations. "Observed with 2⅝ in. telescope." "Keeler's telescope with 2½ in. objective has slight negative spher. aber. and marked deficiency of light in comparison with mine."

June, 1915. "Finished new type of solid solar ocular (i. e., solid with cemented slip of dark glass inside, to be used with Herschel prism.) Highly satisfactory."

During the years 1920, '21, and '22 Hastings acted as optical adviser for the Prisma Company which was experimenting on the production of colored motion pictures. In this connection it may be worthy of note that the friendship which it was my great privilege to share with Hastings grew out of the very lively interest that we both took in the monochrome motion pictures at the time when the art of pantomime and suggested repartee was at the peak of its development.

In a certain letter dated April 23, 1930: "I am about to send you for inspection my 10× ocular and one of my 16 mm 0.25 objectives." "Please note besides its defining power (with high power ocular) its great working distance and the fact that, the front being removed, the back forms an excellent objective of 32 mm 0.12. The last feature is one which, I should imagine, would be of commercial value. My most

[6] See F. E. Beach, *loc. cit.*, 486.

valuable invention, however, if I except my military telescope, is my 16 mm 0.3 objective together with what it promises. It is of three lenses only . . ."

Honors were received by Hastings from many quarters. At the ninth Cincinnati Industrial Exposition he was awarded, on October 8, 1881, a silver medal for his "Telescope Object Glass." On November 19, 1884 he was elected a member of the Connecticut Academy of Arts and Sciences. To this he tendered his resignation in the year 1915. On April 18, 1889 he was elected to membership in the National Academy of Sciences. In the same year he was appointed an *officier de l'instruction publique* in France. As a member of the committee on photographic proofs and apparatus, for the General Paris Exposition of 1889, he received a commemorative diploma on September 29th. At the Paris Exposition of 1900 he was awarded a gold medal on August 18th. He was elected a member of the American Philosophical Society at a meeting held in Philadelphia, Penna., on April 18, 1906. In 1926 the Franklin Institute of Philadelphia awarded Hastings a medal for the improvements he had made in optical instruments. He was a fellow of the American Association for the Advancement of Science, also of the American Physical Society, an honorary member of the Societá degli Spectroscopisti Italiani, and a collaborating editor of the *Astrophysical Journal.* The Physical Club of Yale University was founded in the autumn of 1899, and when, on October 31st, the first meeting was held the management of the club was placed in the hands of an executive committee consisting of Professors J. W. Gibbs, C. S. Hastings, and A. W. Wright.

Hastings' happy disposition and magnetic personality won him many friends not only among scientists but also among cultured people in general. In New Haven he belonged to two distinct sets. The members of one of these represented the University circle and were engaged in intellectual pursuits. The other set was composed in the main of men prominent in banking, law, manufacturing, etc. He was an active member of the select Colby Club, a group which met on alternate Saturday evenings when a member read a half-hour paper on some cultural sub-

ject. For example, a paper by Hastings bore the title: "On Certain Limitations in Science." He was a charter member of the New Haven Lawn Club Association, and president of the exclusive Graduates Club for the three years beginning with 1905.

The ruddy complexion and vigorous health of Hastings were due to his taking plenty of outdoor exercise. He was especially fond of bicycling, often with his daughter, both in this country and in England. He played tennis until fairly late in life, and even after this he continued swimming in season at his summer home in the town of East River, Connecticut. He derived much pleasure and recreation from fishing, particularly on the yachting excursions to southern waters which were made every spring and fall at the invitation of a certain wealthy friend.

In 1878 Hastings married Elizabeth Tracy Smith of Hartford. About three years later their only child was born and baptized as Katherine Panet. The daughter became Mrs. Horace W. Chittenden. She presented her father with four grandchildren, three girls and finally a boy. After a protracted illness Mrs. Hastings died in the fall of 1930. Although Hastings was not a finished musician he did enjoy playing the flute to the piano accompaniment by his wife.

An interesting sidelight is thrown on Hastings' consistent equanimity and contagious cheerfulness by something that was brought out in the course of a discussion on the philosophy of "happiness". His creed was that happiness is a quality which has to be "learned" by one's own efforts. In reply to a recent inquiry of mine Mrs. Chittenden wrote: "He certainly learned it for himself, and even in his last long illness, when so little was left to him, he was almost entirely cheerful and found contentment and happiness in little simple things." Hastings was not a member of any church in New Haven but he often attended services at St. Johns Episcopal Church with his wife and daughter. It is conjectured that he joined a Congregational church when as a youth he dwelt in Hartford, Connecticut. That he pondered over spiritual problems is established by the fact that on several occasions he propounded to me in all seriousness abstruse questions concerning the concept of the

Holy Ghost. On Sunday, January 31, 1932 Hastings died at his daughter's home in Greenwich, Connecticut. On the afternoon of the following Wednesday a small group of us motored to Cedar Hill Cemetery, Hartford, to attend the last services held over the actual ashes of an irreplaceable friend.

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Wallace H Carothers

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WALLACE HUME CAROTHERS

1896–1937

BY

ROGER ADAMS

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1896–1937

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Wallace Hume Carothers, who died on April 29, 1937, was born in Burlington, Iowa, on April 27, 1896. His contributions to organic chemistry were recognized as outstanding and, in spite of the relatively short span of time for his productive accomplishments, he became a leader in his field with an enviable international reputation.

His paternal forbears were of Scotch origin and settled in Pennsylvania in prerevolutionary days. They were farmers and artisans. His father, Ira Hume Carothers, who was born in 1869 on a farm in Illinois, taught country school at the age of 19. Later he entered the field of commercial education and for forty-five years has been engaged in that type of work as teacher and vice-president in the Capital City Commercial College, Des Moines, Iowa. Wallace Hume Carothers was the first scientist in the family.

His maternal ancestors were of Scotch-Irish stock and were also, for the most part, farmers and artisans. They were great lovers of music, and this may account for the intense interest in and appreciation of music which Carothers possessed. His mother, who was Mary Evalina McMullin of Burlington, Iowa, exerted a powerful influence and guidance in the earlier years of his life.

To his sister Isobel (Mrs. Isobel Carothers Berolzheimer), of radio fame as Lu in the trio Clara, Lu and Em, he was especially devoted. Her death in January, 1936, was a staggering shock to him and he was never able to reconcile himself completely to her loss.

On February 21, 1936, he married Helen Everett Sweetman of Wilmington, Delaware. Her father is Willard Sweetman, an accountant, and her mother, Bertha Everett. The family is of English-Welsh descent. Mrs. Carothers received her bachelor's degree in chemistry at the University of Delaware in 1933 and was employed in the patent division of the chemical depart-

ment of the du Pont Company from 1933-1936. A daughter, Jane, was born November 27, 1937.

Wallace was the oldest of four children. His education began in the public schools of Des Moines, Iowa, to which city his parents moved when he was five years of age. In 1914 he graduated from the North High School. As a growing boy he had zest for work as well as play. He enjoyed tools and mechanical things and spent much time in experimenting. His school work was characterized by thoroughness and his high school classmates testify that when he was called upon to recite his answers revealed careful preparation. It was his habit to leave no task unfinished or done in a careless manner. To begin a task was to complete it.

He entered the Capital City Commercial College in the fall of 1914 and graduated in the accountancy and secretarial curriculum in July, 1915, taking considerably less time than the average. He entered Tarkio College, Tarkio, Missouri, in September, 1915, to pursue a scientific course, and simultaneously accepted a position as assistant in the Commercial Department. He continued in this capacity for two years and then was made an assistant in English, although he had specialized in chemistry from the time he entered college. During the World War the head of the department of chemistry, Dr. Arthur M. Pardee, was called to another institution, and Tarkio College found it impossible to secure a fully equipped teacher of chemistry. Carothers, who previously had taken all of the chemistry courses offered, was appointed to take over the instruction. Since he was rejected as a soldier on account of a slight physical defect, he was free to serve in this capacity during his junior and senior years. It is interesting that during his senior year there were four senior chemistry-major students in his class and every one of them later completed work for the doctorate, studying in the universities of this country and abroad. Today they bear testimony to the fact that as undergraduates they owed much to the inspiration and leadership of Carothers.

Upon entering college his interest in chemistry and physical sciences was immediate and lasting, and he rapidly outdistanced his classmates in accomplishment. As a student he showed

mature judgment and was always regarded by his fellow students as an exceptional person. Invariably he was the brightest student in the class regardless of the subject. Financial necessity required that he earn a large portion of his educational expenses. He always found time, however, to associate with the other students, though he showed little interest for the boisterous enthusiasms of the average underclassman. During his last two years in college he was entrusted with a number of student offices to which he gave freely of his time and energy.

Leaving Tarkio College in 1920 with his bachelor of science degree, he enrolled in the chemistry department of the University of Illinois where he completed the requirements for the master of arts degree in the summer of 1921. His former instructor at Tarkio College, then head of the chemistry department at the University of South Dakota, desired a young instructor to handle courses in analytical and physical chemistry and was fortunate in securing Carothers for this position during the school year, 1921-1922. He went to South Dakota only with the intention of securing sufficient funds to enable him to complete his graduate work, but the careful and adequate preparation of his courses, as well as his care of the students under his direction, showed that he could be a very successful teacher of chemistry. He was still the same quiet, methodical worker and scholar, not forceful as a lecturer, but careful and systematic in his contact with the students. He always required adequate preparation of assigned work and was able to get a large volume in student accomplishment.

Simultaneously with his teaching work he started to develop some independent research problems. He was especially interested in the 1916 paper of Irving Langmuir on valence electrons and desired to investigate some of the implications it held in organic chemistry. Pursuing this idea he carried out laboratory studies which were reported in his first independent contribution to the Journal of the American Chemical Society, "The Isosterism of Phenyl Isocyanate and Diazobenzene-Imide." His second independent paper, published while still a student, was that on "The Double Bond." In this he presented the first clear, definite application of the electronic theory to organic

chemistry on a workable basis. He described the electronic characteristics of the double bond and in essence included in his discussion everything that has since been written on this particular subject.

It was evident, even at this stage of his career, that teaching was not his forte. Literally he spent all of his spare time on research problems in which he became interested. A number of his newly found friends in South Dakota tried to induce him to relax somewhat from his constant and sustained application to work, but without avail. He appeared to be driven by the many things that occurred to him as worthy of investigation in the laboratory.

He returned to the University of Illinois in 1922 to complete his studies for the degree of doctor of philosophy, which he received in 1924. His major work was in organic chemistry with a thesis under the direction of Dr. Roger Adams, on the catalytic reduction of aldehydes with platinum-oxide platinum-black and on the effect of promoters and poisons on this catalyst in the reduction of various organic compounds. His minors were physical chemistry and mathematics. He exhibited the same brilliance in all of his courses and in research which characterized his earlier accomplishments. Although specializing in organic chemistry, he was considered by the physical chemists to have a more comprehensive knowledge of physical chemistry than any of the students majoring in that field. In 1920-1921 he held an assistantship for one semester in inorganic chemistry and for one semester in organic chemistry. He was a research assistant during 1922-1923, and during 1923-1924 held the Carr Fellowship, the highest award offered at that time by the department of chemistry at Illinois. During these two years his seminar reports demonstrated his wide grasp of chemical subjects. The frequency with which his student colleagues sought his advice and help was indicative of his outstanding ability. At graduation he was considered by the staff as one of the most brilliant students who had ever been awarded the doctor's degree. A vacancy on the staff of the chemistry department of the University of Illinois made it possible to appoint him as an instructor in organic chemistry in the fall of 1924. In this

capacity he continued with unusual success for two years, teaching qualitative organic analysis and two organic laboratory courses, one for premedical students and the other for chemists.

Harvard University, in 1926, was in need of an instructor in organic chemistry. After carefully surveying the available candidates from the various universities of the country, Carothers was selected. In this new position he taught during the first year a course in experimental organic chemistry and an advanced course in structural chemistry, and during the second year he gave the lectures and laboratory instruction in elementary organic chemistry.

President James B. Conant, of Harvard University, was professor of organic chemistry at the time that Carothers was instructor. He says of him—

"Dr. Carothers' stay at Harvard was all too short. In the brief space of time during which he was a member of the chemistry department, he greatly impressed both his colleagues and the students. He presented elementary organic chemistry to a large class with distinction. Although he was always loath to speak in public even at scientific meetings, his diffidence seemed to disappear in the classroom. His lectures were well ordered, interesting, and enthusiastically received by a body of students only few of whom planned to make chemistry a career. In his research, Dr. Carothers showed even at this time that high degree of originality which marked his later work. He was never content to follow the beaten track or to accept the usual interpretations of organic reactions. His first thinking about polymerization and the structure of substances of high molecular weight began while he was at Harvard. His resignation from the faculty to accept an important position in the research laboratory of the du Pont Company, was Harvard's loss but chemistry's gain. Under the new conditions at Wilmington, he had facilities for carrying on his research on a scale that would be difficult or impossible to duplicate in most university laboratories. Those of us in academic life, however, always cherished the hope that some day he would return to university work. In his death, academic chemistry, quite as much as industrial chemistry, has suffered a severe loss."

In 1928 the du Pont Company had completed plans to embark on a new program of fundamental research at their central laboratory, the Experimental Station at Wilmington, Delaware. Carothers was selected to head the research in organic chemistry. The decision to leave his academic position was a difficult one. The new place demanded only research and offered

the opportunity of trained research men as assistants. This overbalanced the freedom of university life and he accepted. From then on until his death his accomplishments were numerous and significant. He had the rare quality of recognizing the significant points in each problem he undertook, and unusual ability for presenting his results in a most explicit and precise way, which led to clarity and understanding. In these nine years he made several major contributions to the theory of organic chemistry and discoveries which led to materials of significant commercial importance. Dr. Elmer K. Bolton, Chemical Director of the du Pont Company, writes concerning Carothers—

"At the time the du Pont Company embarked upon its program of fundamental research in organic chemistry in the Chemical Department, Dr. Carothers was selected to direct this activity, because he had received the highest recommendations from Harvard University and the University of Illinois, and was considered to have unusual potentiality for future development. There was placed under his direction a small group of excellently trained chemists to work on problems of his own selection. The results of his work, extending over a period of nine years, have been of outstanding scientific interest and have been considered of great value to the Company as they have laid the foundation for several basically new developments of commercial importance.

"In our association with Dr. Carothers, we were always impressed by the breadth and depth of his knowledge. He not only provided inspiration and guidance to men under his immediate direction, but gave freely of his knowledge to the chemists of the department engaged in applied research. In addition, he was a brilliant experimentalist. Regarding his personal characteristics, he was modest, unassuming to a fault, most uncomplaining, a tireless worker—deeply absorbed in his work, and was greatly respected by his associates. He suffered, however, from a nervous condition which in his later years was reflected in poor health and which became progressively worse in spite of the best medical advice and care, and the untiring efforts of his friends and associates. His death has been a great loss to chemistry and particularly to the Chemical Department. In my judgment, he was one of the most brilliant organic chemists ever employed by the du Pont Company."

His reputation spread rapidly; his advice was sought continually, not only by his colleagues but also by chemists throughout the world. In 1929 he was elected Associate Editor of the Journal of the American Chemical Society; in 1930 he became

an editor of Organic Syntheses. He took an active part in the meetings of the organic division of the American Chemical Society. He was invited frequently to speak before various chemical groups. He addressed the Johns Hopkins summer colloquium in 1935 on "Polymers and the Theory of Polymerization." That year he also spoke on the same subject before the Faraday Society in London, when his paper was considered one of the outstanding presentations on the program. His achievements were recognized by his election to the National Academy of Sciences in 1936—the first organic chemist associated with industry to be elected to that organization. During these years from 1928-1937 several attractive academic positions were offered him but he chose to remain to the end with the company which had given him his opportunity for accomplishment.

Very early in life he displayed a love for books. From the time when Gulliver's Travels interest a boy on through Mark Twain's books, Life of Edison, and on up to the masters of English literature, he was a great reader. He possessed a singing voice that might have developed under training into something very worthwhile. Though he had no technical training in music, he was a lover of the great masters, and possessed a large and much-used collection of phonograph records of their works. He said occasionally that were he to start over he would devote his life to music.

Carothers was deeply emotional, generous and modest. He had a lovable personality. Although generally silent in a group of people, he was a brilliant conversationalist when with a single individual, and quickly displayed his broad education, his wide fund of information on all problems of current life, and his critical analysis of politics, labor problems and business, as well as of music, art, and philosophy. With all his fine physique he had an extremely sensitive nature and suffered from periods of depression which grew more pronounced as he grew older, despite the best efforts of his friends and medical advisors.

SCIENTIFIC WORK

His early scientific work involved an extension to organic compounds of Langmuir's idea of isosterism. He demonstrated

that it was valid in the case of phenyl isocyanate and azoimide. Reactions of the double bond were interpreted in terms of the electronic theory, using a point of view that has since gained wide acceptance.

His next efforts were devoted to demonstrating that any idea of "negativity" alone is inherently incapable of accounting for the relative reactivity of organic halides. He measured the base strength of a series of amines. His work on the thermal decomposition of alkali alkyls threw light on the inherent properties of the simplest organic anions.

The first field of which he was in a position to make an exhaustive study was that of acetylene polymers and their derivatives. With vinylacetylene and divinylacetylene made available to him, he completed a detailed study of these substances. It was his discovery that it was possible to add hydrogen chloride to monovinylacetylene with formation of 2-chloro-1, 3-butadiene, called chloroprene. This substance is analogous structurally to isoprene but polymerizes several hundreds of times more rapidly and leads to a product much superior to all previously known synthetic rubbers. It was the first synthetic material to show rubber's curious property of developing fibrous orientation when stretched and instantly reverting to the amorphous condition when released from stress. In resistance to aliphatic hydrocarbons and to most chemical reagents it is definitely superior to natural rubber. It has, moreover, a greater resistance than rubber to corona and sunlight. Carothers' work laid the foundation for the development by other chemists and by chemical engineers of the du Pont Company of the commercial product which has found wide industrial use and which is marketed as neoprene.

These practical results, however, were of no greater importance than the theoretical. In the course of the investigation, many analogs and homologs of chloroprene were prepared and studied. Their behavior threw light on the relationship between the chemical structure of a diene and its suitability as a precursor of rubber. Fundamental information concerning the character and formation of the various polymers from these compounds was revealed and their structures clarified. The reactivity of

the vinylacetylenes and the mechanism by which the products formed was studied in detail. New light was thrown on 1,4 addition and on α,γ rearrangements. His work in this field was a basic contribution to acetylene chemistry.

The most outstanding scientific accomplishment of Carothers was his work on linear polymers. In a letter written to Dr. John R. Johnson of Cornell University on February 14, 1928, Carothers made a statement which demonstrated the careful thought and study which he had given previously to polymerization and polymeric molecules. It follows—

"One of the problems which I am going to start work on has to do with substances of high molecular weight. I want to attack this problem from the synthetic side. One part would be to synthesize compounds of high molecular weight and known constitution. It would seem quite possible to beat Fischer's record of 4200. It would be a satisfaction to do this, and facilities will soon be available here for studying such substances with the newest and most powerful tools.

"Another phase of the problem will be to study the action of substances xAx on yBy where A and B are divalent radicals and x and y are functional groups capable of reacting with each other. Where A and B are quite short, such reactions lead to simple rings of which many have been synthesized by this method. Where they are long, formation of small rings is not possible. Hence reaction must result either in large rings or endless chains. It may be possible to find out which reaction occurs. In any event the reactions will lead to the formation of substances of high molecular weight and containing known linkages. For starting materials will be needed as many dibasic fatty acids as can be got, glycols, diamines, etc. If you know of any new sources of compounds of these types I should be glad to hear about them."

These initial ideas culminated in the publication of a series of thirty-one papers in the field of polymerization. In these he proposed a general theory of condensation-polymerization and a logical and systematic terminology suitable for use in this previously disorganized field. The implications of his theory were illustrated by a series of experimental studies dealing with polyesters, hydrocarbons, polyamides, and polyanhydrides. These studies provided experimental material for correlating chemical structure and physical properties of materials of high molecular weight, and furnished evidence favoring a view now generally accepted for the structure of such natural high poly-

mers as cellulose. In these investigations a new technic—molecular distillation—was applied to the propagation of chemical reactions.

In this study a method new in principle was developed for the synthesis of many-membered cyclic compounds. A large number of many-membered cyclic compounds was synthesized, including several of entirely new types. Some of these compounds had musk-like odors and are otherwise similar in their properties to the genuine musks. One of these new many-membered ring compounds has found industrial application. The large amount of experimental material made possible important deductions bearing on the relationship between chemical structure and ease of ring formation. His contribution was a major one to the field of many-membered ring compounds, which is one of growing significance in organic chemistry.

He investigated the means by which polymers structurally analogous to cellulose and silk could be prepared, and synthesized a large number. These materials constituted the first completely synthetic fibres with a degree of strength, orientation, and pliability comparable with natural fibres. Their study made possible the development of a theory for the relation between structure, fibrous properties, and other physical properties. The work was brilliant and the most important aid in recent years to the understanding of such polymers. This information, and the modification of the physical and chemical properties of polymers by slight changes in the mode of preparation, has made possible the exploration of a wide variety of substances of most promising industrial application.

Based on this work, a commercial development by the chemists and chemical engineers of the du Pont Company has already resulted. An announcement has just been made (October 28, 1938) that the du Pont Company will erect a plant in Seaford, Delaware, which will cost upwards of eight million dollars, for producing a new textile yarn to be known as nylon. This consists of a synthetic fibre-forming polymeric amide with a protein-like chemical structure, characterized by extreme toughness, strength and peculiar ability to be formed into fibres and into various shapes such as bristles and sheets. Filaments of extreme

fineness can be spun, much finer than the filaments of either silk or rayon. One of the more important uses to which nylon will be put is the manufacture of fine hosiery from high-twist nylon yarn. Hosiery made of the new product possesses extreme sheerness, high elasticity, high strength, and improved resistance to runs. Other uses are sewing thread, knit goods, brush bristles, racquet strings, fishing lines and leaders, narrow fabrics, woven dress goods, velvets, plastic compositions, textile finishing agents, and coated fabrics. Exton bristles, the name given to those made from nylon, have already reached the commercial market in the form of "miracle tuft" tooth brushes.

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1995291 Trimethylene carbonate, etc.

1998442 Dichlorobutadiene. (With G. J. Berchet)

2008003 Alkylated lead phenolates.

2012267 Esters of dibasic acids.

2013725 Alcohol addition products of unsaturated compounds such as mono- or divinyl acetylene. (With R. A. Jacobson)

2019118 Laminated glass ("safety glass"). (With G. J. Berchet and R. A. Jacobson)

2020298 Formation of cyclic esters by depolymerizing the corresponding linear polyester. (With J. W. Hill)

2029410 Polymerization products suitable for molded articles, coatings, etc. (With A. M. Collins and J. E. Kirby)

2038538 2-Chloro-1, 3-butadiene, etc.

2061018 Hydrogen polysulfide addition products of unsaturated aliphatic hydrocarbons (resinous products suitable for use in film-forming compositions).

2066329 Polymerizing compounds such as 2-chloro-1, 3-butadiene. (With A. M. Collins and J. E. Kirby)

2066330 Polymerizing 2-chloro-1, 3-butadiene in the presence of vinyl compounds to form rubber-like polymers. (With A. M. Collins and J. E. Kirby)

2066331 Polymerizing 2-chloro-1, 3-butadiene in the presence of α, β-unsaturated aldehydes, ketones, esters, anhydrides, nitriles, etc. (With A. M. Collins and J. E. Kirby)

2067172 Chlorination products of polymerized 2-chloro-1, 3-butadiene, etc.

2068265 Vapor-phase production of aliphatic organic acids. (With G. B. Carpenter)

2071250 Linear condensation "superpolymers" suitable for production of pliable, strong elastic fibers.

2071251 Artificial synthetic fibers from synthetic linear condensation superpolymers.

2071252 Synthetic linear polyacetals by condensation between a glycol and an acetal of a monohydric alcohol.

2071253 Polyamides from amino acids.

2072867 Synthetic rubber and initial materials for its production.

2073363 Butadienyl compounds. (With G. J. Berchet)

Number	
2080558	Dispersing and polymerizing a 2-halo-1, 3-butadiene.
2082568	Vinylacetylene derivatives. (With G. J. Berchet)
2082569	Production of alkyl-vinyl-acetylenes. (With R. A. Jacobson)
2102611	Dichlorobutene and its preparation. (With A. M. Collins)
2104789	Isomerization of isohaloprenes.
2110199	Vinylethynylmethylamines.
2110499	Cyclic acetals.
2124686	Trialkoxybutanes. (With H. B. Dykstra)
2130523	Linear polyamides.
2130947	Diamine-dibasic acid salts.
2130948	Polyamide fibers and methods of making.
2136177	Reaction of chloro-4-butadiene-1, 2 with amines. (With G. J. Berchet)
2136178	Reaction of chloro-4-butadiene-1, 2 with alkaline reacting metal inorganic compounds. (With G. J. Berchet)
2137235	Shaped articles from polymeric materials.

Several other patents will be issued to Doctor Carothers during the next two years.